NBER WORKING PAPER SERIES

A TOUGH ACT TO FOLLOW:
CONTRAST EFFECTS IN FINANCIAL MARKETS

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Working Paper 23883
http://www.nber.org/papers/w23883

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2017

We are grateful to Ross Garon at Cubist Systematic Solutions for helpful suggestions. We thank Yiran Fan, Menaka Hampole, Michael Watson, and Linda Ye for excellent research assistance. We thank John Beshears, Justin Birru, James Choi, Diego Garcia, Nicola Gennaioli, Wesley Gray, Terrence Hendershott, Owen Lamont, Bill Mayew, Quoc Nguyen, Josh Schwartzstein, Andrei Shleifer, Doug Skinner, Eric So, David Solomon, and Boris Vallee as well as seminar participants at AFA Annual Meeting, AQR Insight Award Meeting, Arrow Street Capital, BEAM, City University of Hong Kong, Chicago Booth, Colorado Finance Summit, Cubist Systematic Solutions, FIRN, Frontiers in Finance Conference, FSU Suntrust Finance Conference, Gerzensee ESSFM, Harvard Economics, Helsinki Finance Summit, IDC, London Business School, Maastricht University, MIT Sloan JFFC, NBER Behavioral Finance, Tilburg University, UBC Winter Finance Conference, and the Wharton Jacobs Levy Forum for comments. We thank Sam Hanson and Adi Sunderam for sharing data. The authors have nothing to disclose. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 23883
September 2017
JEL No. D03,D9,D91,G12,G14,G4

ABSTRACT

A contrast effect occurs when the value of a previously-observed signal inversely biases perception of the next signal. We present the first evidence that contrast effects can distort prices in sophisticated and liquid markets. Investors mistakenly perceive earnings news today as more impressive if yesterday's earnings surprise was bad and less impressive if yesterday's surprise was good. A unique advantage of our financial setting is that we can identify contrast effects as an error in perceptions rather than expectations. Finally, we show that our results cannot be explained by a key alternative explanation involving information transmission from previous earnings announcements.

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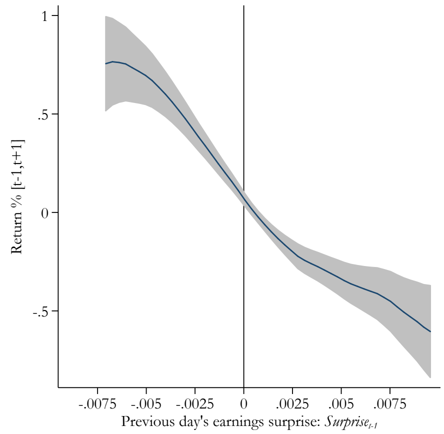
A data appendix is available at http://www.nber.org/data-appendix/w23883

Figure 1



The value-weighted earnings surprise of large firms that announced earnings in the previous trading day versus the return of firms that announced earnings today (conditional on own earnings surprise).

> ***Socrates:*** *Could you tell me what the beautiful is?*
> ***Hippias:*** *For be assured Socrates, if I must speak the truth, a beautiful maiden is beautiful.*
> ***Socrates:*** *The wisest of men, if compared with a god, will appear a monkey, both in wisdom and in beauty and in everything else. Shall we agree, Hippias, that the most beautiful maiden is ugly if compared with the gods?*
>
> -Plato

People often interpret information by contrasting it with what was recently observed. For example, Pepitone and DiNubile (1976) show that subjects judge crimes to be less severe following exposure to narratives of very egregious crimes. Kenrick and Gutierres (1980) show that male students rate female students to be less attractive after viewing videos of beautiful actresses. References to such "contrast effects" are also pervasive in our popular culture. People complain about having "a tough act to follow" when they are scheduled to perform following a great performance. Writers use literary foils to exaggerate a character's traits through juxtaposition with a contrasting character. Fashion designers use shoulder pads and peplum hips to create the illusion of a comparatively smaller waist. In all of these cases, contrast effects bias our perception of information. We perceive signals as higher or lower than their true values depending on what else was recently observed.

Contrast effects have the potential to bias a wide variety of important real-world decisions. They may distort judicial perceptions of the severity of crimes, leading to unfair sentencing. At firms, comparisons with the previously reviewed candidate could lead to mistakes in hiring and promotion decisions. An unconstrained firm may pass on a positive NPV project because it does not look as good as other options or invest in a negative NPV project because it looks better than even worse alternatives. Finally, at the household level, contrast effects could cloud key decisions such as mate choice and housing search.

In these examples, contrast effects potentially lead to costly mistakes, but it may be difficult for researchers to cleanly measure the bias. Measurement is complicated by the possibility that the decision-makers face unobserved quotas or resource constraints that make comparisons across multiple cases optimal. In addition, researchers often lack precise data on how decision-makers perceive information. Possibly because of these challenges, most of the existing research about contrast effects has focused on controlled laboratory experiments. Evidence from the field is more limited. Outside of the lab, Bhargava and Fisman (2014) show contrast effects in mate choice using a speed dating field experiment, and Simonsohn and Loewenstein (2006) and Simonsohn (2006) show contrast effects in consumer housing and commuting choices.

Our paper tests whether contrast effects operate in another important real world setting: financial markets. The financial setting is particularly interesting because we can test whether contrast effects distort equilibrium prices and capital allocation in sophisticated markets. Full-time professionals making repeated investment decisions may be less prone to such a bias than individuals making infrequent dating or real estate decisions. Moreover, the limited field evidence examines contrast effects in household decision-making, but prices in financial markets are determined through interactions among many investors. Thus, cognitive biases among a subset of investors may not affect market prices given the disciplining presence of arbitrage. And yet, if contrast effects influence prices in financial markets, they would represent an important form of mispricing: prices react not only to the absolute content of news, but also to a bias induced by the relative content of news.

In this paper, we test whether contrast effects distort market reactions to firm earnings announcements. Quarterly earnings announcements represent the main recurring source of news released by publicly-traded US firms. Prior to the announcement, financial analysts and investors form expectations of what they believe earnings will be. Earnings surprises, i.e., the extent to which actual earnings exceed or fall short of those expectations, are associated with stock price movements because they represent new information that shifts expectations of firm prospects. Earnings announcements are typically scheduled weeks beforehand, so whether a given firm announces following positive or negative surprises by another firm is likely to be uncorrelated with the firm's fundamentals.

The theory of contrast effects predicts a *negative* relation between yesterday's surprise and the return reaction to today's earnings surprise, holding today's earnings surprise constant. The intuition is that today's news will seem slightly less impressive than it would otherwise if yesterday's earnings surprises were positive and slightly more impressive if yesterday's earnings surprises were disappointing. While an earnings surprise is a concrete number (e.g., earnings per share was $0.14, beating analyst forecasts of $0.10, translating to a positive surprise of $0.04), there is significant subjectivity in translating a surprise into a return response. A positive surprise is good news, but *how much* the price goes up depends on the interpretation of what the surprise implies for the future of the firm. We test whether the perception of "how good" the good news is (or "how bad" the bad news is) is biased by contrast effects.

The downward sloping pattern in Figure 1 illustrates our main finding. The figure shows a local linear plot of returns surrounding a firm's earnings announcement relative to the value-weighted average earnings surprise announced by large firms in the previous trading day, hereafter referred to as $surprise_{t-1}$. The figure demonstrates a strong negative relation: controlling for today's earnings news, the return reaction to today's earnings announcement is inversely related to $surprise_{t-1}$. The effect is sizable—a change in yesterday's earnings surprise from the worst to the best decile corresponds to a 53 basis point lower return response to today's earnings announcement.

We find evidence of a simple directional effect whereby a high surprise yesterday makes *any* surprise today (even more positive surprises) look slightly worse than it would appear if yesterday's surprise had been lower. In other words, the magnitude of the return distortion depends strongly on yesterday's surprise and not significantly on the interaction between today's and yesterday's surprise. Visually, this manifests as a vertical shift downward in the return response curve to the firm's own earnings surprise if yesterday's news was good rather than bad (see Figure 3).

While our findings are consistent with the theory of contrast effects, one may be concerned that we are capturing a reaction to information transmitted from earlier earnings announcements. Rational reaction to information released at time $t-1$ implies that the information is quickly incorporated into prices, and therefore will have no predictive power for future returns. We can reject such explanations by showing that $surprise_{t-1}$ negatively predicts the future returns of firms scheduled to announce on day t, *without conditioning on day t earnings news*. This allows us to create a simple trading strategy where we go long (short) firms scheduled to announce today if yesterday's surprise was low (high). Executing this trading strategy for firms in the top quintile of size yields abnormal returns of roughly 15% per year, suggesting that, unlike many anomalies, contrast effects can distort the returns of large firms. We also show that the market does not respond as if $surprise_{t-1}$ contains information that is relevant for firms scheduled to announce the following day. The prices of firms scheduled to announce on day t do not move on day $t-1$ in response to day $t-1$ news. Further, $surprise_{t-1}$ does not predict the next day's announcements after controlling for slower moving time trends.

We also find that the return distortions caused by contrast effects reverse over the next 50 trading days, consistent with mispricing that is eventually corrected. This reversal also helps us to reject an alternative explanation in which investors have a delayed reaction to information transmitted from earlier announcements. Rational processing of information, with or without a delay, should not lead to the return reversals observed in the data.

We explore a variety of other alternative potential explanations and do not find support for them in the data. After examining a number of proxies for limits to arbitrage capital, we find no evidence that it is a dominant mechanism underlying the empirical patterns. Fixed firm-specific loadings on risk factors are also unlikely to explain our results because we use characteristic-adjusted returns in our analysis. Further, we find that daily changes in risk loadings, return volatility, volume, and other measures of trading frictions do not vary by $surprise_{t-1}$. A final potential concern is that firms strategically advance or delay their earnings announcements or manipulate the earnings announcement itself (e.g., Sloan, 1996; DellaVigna and Pollet, 2009; and Johnson and So, 2017), but we find similar results within subsamples of firms that are unlikely to have engaged in this behavior.

In supplementary analysis, we explore the timing and boundaries of how contrast effects manifest in financial markets. In much of the experimental evidence concerning sequential contrast effects, recency (i.e., the signal seen in the most recent experimental period) is the dominant factor for contrast effects (e.g., in speed dating Bhargava and Fisman, 2014; olympic judging Damisch et al., 2006; and ratings of attractiveness Kenrick and Gutierres, 1980). However, it is an open question whether the same recency predictions apply to investor perceptions of earnings, and what time interval investors treat as "recent." When examining contrast effects across one or more trading days, we find that investor perceptions of earnings announced today are negatively affected by earnings surprises released by other firms on $t-1$, but are not significantly affected by lagged earnings surprises on $t-2$ and $t-3$ or future earnings surprises on $t+1$ and $t+2$. Within a trading day, we find that contrast effects from morning salient announcements significantly bias return reactions to announcements made in the afternoon. Finally, we find suggestive evidence that investors in smaller firms pay more attention to previous announcements by other firms in the same industry. Meanwhile, investors in larger firms pay attention to the recent earnings announcements of other large firms, but pay relatively more attention to same-industry announcements if such a comparison is available.

One of the main contributions of our paper is to further the understanding of how psychological biases found in the lab manifest in real-world settings (e.g., Levitt and List, 2007a,b). Our analysis shows that contrast effects persist in a market setting where prices are determined by interactions among many investors, including potentially deep-pocketed arbitrageurs. Our findings also highlight a cognitive bias that has not previously been explored in the behavioral finance literature and that predicts distinct patterns for mispricing. A large branch of the existing behavioral finance literature focuses on limited attention, in which investors underreact to news that is hidden, difficult to process, or obscured by other more attention-grabbing events (e.g., Barber and Odean, 2008).[1] Contrast effects differ sharply from limited attention because contrast effects are strong only if investors pay attention to the signals being contrasted. In other words, salience increases rather than decreases contrast effects bias. This helps explain why we find a strong contrast effects bias even for large firms with salient earnings announcements, while the previous literature on limited attention applies primarily to small firms, or news obscured in footnotes or released after hours on Fridays.

A second large branch of the behavioral finance literature studies expectational errors, such as over-extrapolation or the gambler's fallacy (e.g., Greenwood and Shleifer, 2014). We are able to identify contrast effects as a perceptual error rather than an expectational error. The distinction is mainly in regard to *when* an agent makes a quality assessment. Under a perceptual error such as contrast effects, agents hold a biased quality assessment about the next case only *after* seeing the next case. Under an expectational error, seeing one case causes agents to hold mistaken beliefs about the quality of future cases, before the future cases are directly observed. For example, in the context of earnings, Thomas and Zhang (2008) show an expectational bias in which investors overreact to industry-specific news released early in an earnings season. This expectational error is corrected when a firm announces its actual earnings. Since we find evidence of price distortions for the second firm to announce only after it announces its own earnings, our evidence is consistent with a perceptual bias rather than an expectational bias. Distinguishing between these two types of errors is important because they imply distinct return patterns. Expectational errors lead to mistaken predictions about future outcomes that can be corrected once the outcome is realized. Perceptual errors lead to persistent mispricing even after the outcome is realized and news is revealed.

Our evidence also underscores how important decisions are distorted by comparisons to benchmarks that should be irrelevant. Thus, our research is related to a large theory literature on

[1] Much of the existing research on biased reactions to earnings announcements is also related to limited attention. It has been shown that investors underreact to a firm's own earnings news (Ball and Brown, 1968; Bernard and Thomas, 1989, 1990; and Ball and Bartov, 1996), predictable seasonal information (Chang et al., 2016), and information in the timing of announcements (DellaVigna and Pollet, 2009; Johnson and So, 2017; Boulland and Dessaint, 2017).

context-dependent choice and reference points (e.g., Kahneman and Tversky, 1979; Koszegi and Rabin, 2006, 2007; Kamenica, 2008; Cunningham, 2013; Bordalo et al., 2015; and Bushong et al., 2015).[2] Finally, our findings are related to research in household finance examining investor behavior based on how positions performed since they were purchased (Shefrin and Statman, 1985; Odean, 1998) and how a position compares to the other holdings in an investor's portfolio (Hartzmark, 2015). Relative to this literature which focuses on the trading patterns of individual investors, we test how contrast effects in the perception of news affect equilibrium market prices for large cap stocks.

1 Data

1.1 Sources

We use the I/B/E/S detail history file for data on analyst forecasts as well as the value and dates of earnings announcements. The sample is restricted to earnings announced on calendar dates when the market is open. Day t refers to the date of the earnings announcement listed in the I/B/E/S file.[3] Day $t-1$ refers to the most recent date prior to t when the market was open. We examine the quarterly forecasts of earnings per share and merge this to information on daily stock returns from CRSP and firm-specific information from Compustat. Data on the market excess return, risk-free rate, SMB, HML, UMD, and short term reversal portfolios as well as size cutoffs come from the Kenneth French Data Library.

To account for standard risk-based return movements, we use characteristic-adjusted returns, i.e., raw returns in excess of the return of a portfolio of stocks with similar characteristics. We follow the procedure in Daniel et al. (1997) and sort stocks into NYSE quintiles based on size, book value of equity divided by market value of equity (calculated as in Fama and French, 1992), and

[2]While closely related to this literature, contrast effects (as typically described in the psychology literature) refer to a simple directional phenomenon in which the value of the recently observed signal inversely affects perception of the next signal. Most descriptions of contrast effects do not require discontinuous or kinked responses around a reference point (as in prospect theory, with recent empirical applications in, e.g., Baker et al., 2012 and DellaVigna et al., 2016) or a choice framework to identify which reference points to use or where to allocate attention.

[3]DellaVigna and Pollet (2009) highlight a potential concern regarding earnings announcement dates as reported in I/B/E/S. We address this in Section 4 and show that alternative date adjustments lead to similar results.

momentum calculated using returns from $t-20$ to $t-252$ trading days (an analogue to a monthly momentum measure from months $m-2$ to $m-12$). We then match each stock's return to the portfolio of stocks that match these three quintiles of characteristics.

We introduce one modification to ensure that there is no mechanical relation between the returns of the characteristic-matched portfolio and $surprise_{t-1}$, our measure of the salient surprise released on day $t-1$. We remove from the characteristic-matched portfolio a stock's own return and the return of firms included in the calculation of $surprise_{t-1}$. This ensures that the characteristic-adjusted return is not distorted by potential earnings-related drift in the return of stocks that announced on the previous day.[4] Our measure of return on day t is a stock's raw return on day t minus the day t return of this characteristic-matched portfolio. In the remainder of the paper, unless otherwise noted, we refer to these characteristic-adjusted returns as returns.

We measure the close-to-close return for day t as the return from market close on day $t-1$ to market close on day t. We measure the open-to-open return for day t as the return from market open on day t to market open on day $t+1$. The analysis uses close-to-close returns unless open-to-open is specified.

1.2 Measuring earnings surprise

A key variable in our analysis is the surprise for a given earnings announcement.[5] Broadly defined, earnings surprise is the difference between announced earnings and the expectations of investors prior to the announcement. We follow a commonly-used method in the accounting and finance literature and measure expectations using analyst forecasts prior to announcement. This measure is available for a long time-series and does not require us to take a stand on specific modeling

[4] We thank James Choi for suggesting this modification to the calculation of characteristic-adjusted returns. In the Internet Appendix, we show that using raw returns in excess of the market or standard characteristic-adjusted returns without this correction yields similar results.

[5] We follow the literature on earnings announcements in characterizing earnings news as the surprise relative to expectations. We focus on surprise rather than levels because whether a given level of earnings is good or bad news depends on the level relative to investor expectations. In addition, stock prices should reflect current information—the stock market return response to an earnings announcement represents the change in a firm's valuation, which should depend on the difference in earnings relative to expectations. Moreover, the financial press typically reports earnings announcement news in terms of how much earnings beat or missed forecasts. Therefore, the earnings surprise is likely to be the measure of earnings news that is most salient to investors.

assumptions (for example, assuming that expectations follow random walk with drift as in Bernard, 1992). Analysts are professionals who are paid to forecast future earnings. While there is some debate about how unbiased they are (e.g., McNichols and O'Brien, 1997; Lin and McNichols, 1998; Hong and Kubik, 2003; Lim, 2001; and So, 2013), our tests only require that such a bias is not correlated with the surprises of other firms in the day before a firm announces earnings. Given that we only use forecasts made before the $t-1$ firm announces (forecasts from day $t-2$ or earlier), such a bias is unlikely to exist.

Similar to DellaVigna and Pollet (2009), we take each analyst's most recent forecast, thereby limiting the sample to only one forecast per analyst, and then take the median of this number within a certain time window for each firm's earnings announcement. In our base specification, we take all analyst forecasts made between two and fifteen days prior to the announcement of earnings. We choose fifteen days to avoid stale information yet still retain a large sample of firms with analyst coverage. To show that these assumptions are not driving the results, we present variations of this measure in the Internet Appendix utilizing longer windows of 30 and 45 days prior to announcement and also using the return reaction to the announcement as a measure of earnings surprise.

We follow DellaVigna and Pollet (2009) and scale the difference between the actual surprise and the median analyst forecast by the share price of the firm from three trading days prior to the announcement. Thus, our estimate of the earnings surprise for firm i on day t can be written as:

$$surprise_{it} = \frac{\left(actual\ earnings_{it} - median\ estimate_{i,[t-15,t-2]}\right)}{price_{i,t-3}}. \tag{1}$$

Scaling by price accounts for the fact that a given level of earnings surprise implies different magnitudes depending on the price per share. For example, a five cent surprise represents a bigger positive surprise if the stock price is valued at \$10/share than \$100/share. However, many media outlets report earnings surprises as the unscaled difference between actual earnings and analyst forecasts, and investors may pay attention to the unscaled surprise. In Section 4, we find qualitatively similar results using the unscaled earnings surprise.

To test the contrast effects hypothesis, we need a measure of the surprise occurring on the previous day taking into account that multiple firms may have announced earnings. The ideal variable would focus on the earnings announcements in $t-1$ that were salient, as this would be the comparison group in the minds of investors when they evaluate the current day's announced earnings. While we do not have an exact measure of the salient surprise in $t-1$, we utilize a number of proxies and focus most of our analysis on large firms. A firm's market capitalization is related to how much attention that firm receives. One measure we use is simply the surprise of the largest firm to announce on day $t-1$. A second measure, which we use as our baseline, is the value-weighted surprise, using each firm's market capitalization three days prior to the firm's announcement, among all large firms announcing on day $t-1$. We define large firms as those with market capitalization (measured three days before the firm's announcement) above the NYSE 90th percentile of market capitalization in each month.[6] Thus, our baseline measure of yesterday's salient surprise is:

$$surprise_{t-1} = \frac{\sum_{i=1}^{N} (mkt\,cap_{i,t-4} \times surprise_{i,t-1})}{\sum_{i=1}^{N} mkt\,cap_{i,t-4}} \qquad (2)$$

To reduce the influence of outliers, we winsorize $surprise_{it}$ at the 1st and 99th percentile and take the weighted average to create $surprise_{t-1}$. After creating $surprise_{t-1}$, we again winsorize at the 1st and 99th percentiles. In later regression analysis, each observation represents an earnings announcement by firm i on day t. When we discuss $surprise_{t-1}$, we refer to the salient earnings surprise released by large firms on the previous trading day.

1.3 Summary statistics

Table 1 describes the data used in our baseline specification. Our sample begins in 1984 and ends in 2013. For our main analysis, we examine how the return reaction for a firm that announces earnings

[6]The robustness of the results to this somewhat ad hoc cutoff is explored in Section 4. We present alternative formulations for $surprise_{t-1}$ in which we use size cutoffs at the 85th and 95th percentiles or weight firms that announced in $t-1$ by volume or analyst coverage.

on day t relates to the salient earnings surprise of other firms released on day $t-1$, controlling for the firm's own earnings surprise. Thus, to be included in the sample, a firm must have at least one analyst forecast in our dataset between days $t-2$ and $t-15$ prior to the announcement. In addition, we require a non-missing measure of $surprise_{t-1}$, which means at least one firm above the 90th percentile of market-capitalization announced their earnings on day $t-1$ and at least one analyst forecasted earnings for this firm between days $t-16$ and $t-3$. After applying these filters and requiring the firm with an announcement on day t to have non-missing returns, we are left with 75,897 unique earnings announcements.

We see that days with an earnings announcement are associated with positive returns (in excess of the matched characteristic portfolio), with a mean and median of 18 and 7 basis points, respectively. This is the earnings announcement premium described in Beaver (1968), Frazzini and Lamont (2007), and Barber et al. (2013). Table 1 also shows that the typical earnings surprise is approximately zero (a mean of -0.0003 and a median of 0.0002). The market cap row shows the mean market capitalization in our sample is roughly \$7.7 billion, while the 25th percentile of market cap is \$440 million, implying that we have many small firms in our sample. Our baseline analysis will focus on larger firms because we value-weight each observation. We find a similar pattern when examining analyst coverage (number of forecasts from $t-15$ to $t-2$). For many firms, we see only one analyst forecast and the median number of forecasts is two, while the mean number of forecasts is nearly four. Thus, a small number of firms are covered heavily by many analysts. The final row describes the number of firms used to construct $surprise_{t-1}$, that is firms above the 90th percentile that announced on the previous trading day. The median is 6 with a mean of 7.6, so in general multiple firms comprise the $surprise_{t-1}$ measure.

2 Results

2.1 Baseline results

In our simplest specification, we test how the return response to a given earnings surprise is impacted by the earnings surprise announced by large firms on the previous trading day. Under the null hypothesis of frictionless efficient markets, it is not possible to predict future returns using past publicly available information. On the other hand, the theory of contrast effects predicts that the salient surprise on $t-1$ ($surprise_{t-1}$) will negatively affect the return reaction to announcements made on day t. Therefore, we estimate the following regression:

$$return_{i,[t,t+1]} = \beta_0 + \beta_1 \cdot surprise_{t-1} + \varepsilon_{it}. \tag{3}$$

The dependent variable is the characteristic-adjusted return from market open on day t to market open on day $t+2$ for a given firm i that announces its earnings on day t. In all regressions, unless otherwise noted, we value-weight each observation using firm i's market capitalization three days prior to the firm's announcement, scaled by the average market capitalization in that year, in order to focus on the more economically meaningful firms.[7] We cluster the standard errors by date.

$Surprise_{t-1}$ is our measure of yesterday's salient earnings surprise and β_1 is our main coefficient of interest. All information contained in $surprise_{t-1}$ is announced prior to when our left hand side return measure begins. Thus, in a frictionless efficient market, β_1 should be equal to zero. The contrast effects hypothesis instead predicts that a high surprise yesterday makes any surprise today look slightly worse than it would appear if yesterday's surprise had been lower. Thus, contrast effects predict a negative coefficient on β_1.

In Table 2, we estimate that β_1 is negative and highly significant. For our first estimate of yesterday's salient surprise in Column 1, we use the earnings surprise of the largest firm to announce

[7] Average market capitalization has increased over time. To avoid mechanically overweighting recent observations, we scale market capitalization by the average in each year.

in the previous day. To make sure this firm is salient, we include only observations where the firm is above the 90th percentile of NYSE market capitalization. We estimate a significant β_1 of -0.526. Examining only the largest firm is a coarse measure of the salient earnings surprise from the previous day if there were multiple large firms that announced. For example, if both Apple and Goldman Sachs announced earnings on the same day, both announcements may be salient to investors. Column 2 of Table 2 measures $surprise_{t-1}$ using the equal-weighted mean of all firms that announced in the previous day and were large (above the 90th percentile of market capitalization). We estimate a significant β_1 of -0.846. Finally, Column 3 uses the value-weighted mean of the earnings surprise of all large firms that announced yesterday, leading to a significant β_1 of -0.780. This value-weighted measure implicitly assumes that the relative market cap of large firms that announced on $t-1$ is a good proxy for the relative salience of their announcements. The results in the first three columns of Table 2 show that returns are predictable based upon publicly available past information. This return predictability allows us to reject a model of frictionless efficient markets in which investors rationally process any information from earlier earnings announcements.

The negative β_1 coefficient is consistent with the contrast effects hypothesis, but Equation 3 is not the most direct test for contrast effects. In particular, a negative β_1 in Equation 3 is also consistent with an alternative behavioral explanation where investors have mistaken beliefs about what $surprise_{t-1}$ implies for a firm announcing earnings on day t. Investors may over-infer that a positive $surprise_{t-1}$ is good news for this firm, leading to positive returns on day $t-1$ and then a negative return correction on day t when the firm's earnings are actually announced. To more directly test the contrast effect hypothesis, we estimate the following regression:

$$return_{i,[t-1,t+1]} = \beta_0 + \beta_1 \cdot surprise_{t-1} + own\ surprise\ bin + \varepsilon_{it} \tag{4}$$

This regression modifies Equation 3 in two ways. First, we extend the return window for firm i to include the period prior to when $surprise_{t-1}$ is announced ($return_{i,[t-1,t+1]}$ is measured starting at market close on $t-2$). Given this longer return window, a negative coefficient β_1 cannot be caused by overreaction to news on day $t-1$ and a subsequent correction when firm i's news is released on day t. An additional benefit of extending the return window to before $surprise_{t-1}$ is announced is that we can use standard close-to-close returns data, which is available for a longer time period than open-to-open returns. Second, we control for firm i's own earnings surprise. A major determinant of the return response to any earnings announcement will, of course, be the earnings surprise that the firm actually announces. Contrast effects predict that, conditional on the actual earnings surprise, perception of that news will be too high or low depending on the salient surprise released the previous day.[8] We flexibly control for the firm's own earnings surprise with *own surprise bin*, which represents twenty equally-sized bins based on the size of the earnings surprise with an additional indicator for a surprise of zero. By using dummy variables for each bin, we non-parametrically allow each magnitude of surprise to be associated with a different level of average return response. With these changes, $\beta_1 < 0$ is direct evidence of contrast effects.

Columns 4, 5, and 6 of Table 2 show the estimates of β_1, using the modified regression with the extended return window and own surprise controls. In all specifications, we estimate a significant and negative β_1 that is of similar magnitude to earlier estimates. For the remainder of the paper, we refer to Column 6, which utilizes the value-weighted measure of $surprise_{t-1}$ as our baseline specification. Using the β_1 of -0.924 from Column 6, we estimate that an increase in yesterday's salient surprise from the average in the worst decile (-0.21%) to the average in the best decile (0.38%) is associated with lower returns of 55 basis points, holding $i's$ actual news constant. Alternatively, a one standard deviation increase in $surprise_{t-1}$ is associated with a decrease in returns of 16 basis points. To get a sense of magnitudes, we can compare this result to a robust anomaly in asset pricing: the earnings announcement premium (Frazzini and Lamont, 2007; Barber et al., 2013). In our sample, with no information other than that earnings will be announced on a given day, an equal-weighted strategy long stocks with earnings announcements earns abnormal returns of 17

[8] Controlling for firm i's actual news allows for a direct test of the contrast effects hypothesis: previous signals inversely bias perception of the next signal relative to its true value. Note that the contrast effects hypothesis does not require zero autocorrelation in true signal values, just that perceptions of the next signal are inversely biased by the previous signal. In practice, we will show that earnings surprises display no significant day-to-day autocorrelation after accounting for slower moving time trends.

basis points from $t-1$ to $t+1$. If we value-weight, as we do for our estimates of contrast effects, the earnings announcement premium is 8 basis points from $t-1$ to $t+1$. Thus, the impact of contrast effects is of a similar magnitude to, if not greater than, the earnings announcement premium.

Figure 2 shows the graphical analogues to the regressions in Table 2. Panel A mirrors Equation 3 and shows that $surprise_{t-1}$ strongly negatively predicts future returns of firms scheduled to announced the next day. Panel B mirrors Equation 4 and shows that the same negative relation exists if we use an extended return window and control for the firm's own earnings surprise. Both panels are consistent with contrast effects inducing a negative relation between yesterday's salient surprise and the return reaction to today's earnings surprise.

The contrast effects hypothesis predicts that the return response to a given earnings surprise today will be higher when yesterday's news was bad than when yesterday's news was good. In its simplest form, the magnitude of the bias depends on the value of yesterday's surprise, but not on whether today's surprise is better or worse than yesterday's surprise. This simple directional effect can be seen in Figure 3, which shows how $surprise_{t-1}$ shifts the return reaction curve to the firm's own earnings surprise. The blue and red lines show the return response for firms that announce following a very positive (top decile) or negative (bottom decile) $surprise_{t-1}$, respectively. Unsurprisingly, for both groups, there is a strong positive relation between a firm's returns around announcement and the firm's own earnings surprise. More importantly, the figure shows that the blue line is consistently below the red line, demonstrating that the return response to a firm's own earnings surprise is shifted down significantly if yesterday's surprise was in the highest decile as compared to the lowest decile. The figure also shows that the magnitude of the contrast effect, i.e., the vertical distance between the two lines, does not vary substantially across the support of earnings surprises released today. In other words, good salient surprises yesterday makes all earnings surprises today (even more positive earnings surprises) look slightly less impressive than if they had followed bad salient surprises yesterday, and the magnitude of this difference does not differ substantially based on the level of surprise released today.

We test directly for potential interaction effects in Table 3 by interacting $surprise_{t-1}$ with various measures of the firm's own earnings surprise: the raw level, 20 bins, and quintiles for the firm's own earnings surprise. We find that the magnitude of contrast effects may be slightly larger when the surprise today is more negative, but the interaction effects are all insignificantly different from zero. Further, we continue to find a strong negative direct relation between returns and the previous day's salient surprise, even after we allow for interaction effects. In other words, yesterday's salient surprise negatively impacts the return reaction to today's earnings announcement, and the extent of this distortion does not depend significantly on the level of today's earnings surprise. Therefore, we focus on the direct effect, but do not claim to reject potential interaction effects which may be too noisy to estimate within our sample.

2.2 Long run reversals

If the return patterns represent mispricing due to the psychological bias of contrast effects, then the negative coefficient on $surprise_{t-1}$ should reverse over time if prices eventually converge to fundamental values. Table 4 examines returns subsequent to the earnings announcement and finds evidence of significant reversals within 50 trading days. The first column replicates our baseline specification which focuses on immediate return reactions. Columns 2-4 in the top row looks at the overall impact of $surprise_{t-1}$ on longer run return windows, starting at $t-1$ up to $t+75$. We find that the contrast effect persists up to 25 trading days but becomes insignificantly different from zero by 50 trading days after announcement.

In the Columns 5-7, we focus directly on the reversal period by examining return windows starting at $t+2$, after the initial reaction period. Looking only at the post-event window means that a reversal should manifest itself as a positive coefficient on $surprise_{t-1}$. In the initial 25 trading days, we don't see significant movements in prices. Examining the period from $t+26$ to $t+50$, we see strong significant positive effects of roughly the same absolute magnitude as the baseline effects from $t-1$ to $t+1$. Finally, examining the next period from $t+51$ to $t+75$ we find no significant further reversal. Thus, the evidence suggests that contrast effects lead to mispricing that is reversed in approximately 50 trading days after announcement.

2.3 Trading strategy

One important implication of the initial results that do not condition on a firm's own earnings surprise is that we can predict day t and future returns using information available on day $t-1$. Thus, it would be possible to trade based on the previous day's salient earnings surprise and earn predictably higher or lower returns for firms that are scheduled to announce earnings the next day. We construct a simple calendar-time trading strategy based on contrast effects. The purpose of this analysis is not to find the maximum alpha attainable to traders, but rather to show the results are robust to a different specification. Calendar time asset pricing offers a different risk adjustment than the characteristic-adjusted returns used elsewhere in the paper. In addition, the trading strategy uses daily diversified value-weighted portfolios that more closely resemble what investors might hold. The strategy equal-weights trading days (and value-weights multiple earnings announcements within the same day) while the baseline regressions value-weight each earnings announcement.

The trading strategy is a daily long-short strategy. On days where the salient surprise at $t-1$ is below the 25th percentile of $surprise_{t-1}$ (relative to the distribution of $surprise_{t-1}$ in the previous quarter), we buy firms with an earnings announcement on day t. On days where $surprise_{t-1}$ is above the 25th percentile of $surprise_{t-1}$ over the previous quarter, we short stocks with an earnings announcement on day t. The position is held for days t to $t+1$ beginning at market open on day t. If this strategy is only active in the long (short) leg on a given day, we short (long) the market for the other leg. The portfolios are value-weighted based upon the market capitalization at $t-3$ of the firms announcing earnings on day t. We form portfolios using only large firms, in the top quintile of the market, that account for our findings (see Table 11). We regress portfolio returns on the market, size, book to market, momentum, and short term reversal factors.

Table 5 presents the results. Column 1 shows that the strategy yields a five-factor alpha of 19 basis points with a t-statistic greater than 3. We can compound these daily alphas to estimate the annual alpha of a contrast effects trading strategy. If the trading strategy could be implemented every trading day (which is not the case), 19 basis points per day would yield an annual abnormal return of over 45%. However, earnings announcements cluster within each quarter, and not all trading days contain earnings announcements. Further, the strategy can only be implemented if there is a non-missing salient surprise in the top or bottom quartile of $surprise_{t-1}$ in the previous trading day. In our sample, we can implement the strategy for an average of 76 trading days per year (roughly 30% of total trading days), which yields an abnormal annual return of 15%.

Whether the trading strategy continues to yield positive abnormal returns after accounting for trading costs depends heavily on assumptions regarding these costs, an issue that we do not take a strong stand on in this paper. Following the procedure from Breen et al. (2002) (which serves as the basis for Mitchell and Pulvino, 2001) to estimate direct and indirect trading costs, we find that trading costs for the average position are roughly equal to the level of the alpha, and would wipe out the gains from the trading strategy.[9] In contrast, Frazzini et al. (2012) directly examine trading costs for a large institutional trader during a time period that overlaps more with our sample and estimate much lower trading costs. Using their median estimates of 8 to 10 basis points, the contrast effects trading strategy would remain profitable. However, we caution that the estimates in Frazzini et al. (2012) are based on the implementation of a different set of anomaly strategies and may not account for the decrease in liquidity prior to earnings announcements, which could raise trading costs (So and Wang, 2014). Finally, institutional investors would likely engage in more detailed analysis to further increase the alpha and minimize trading costs, while our analysis intentionally uses a very simple trading rule to form portfolios.

In addition to a trading strategy exploiting the direct impact of contrast effects on short run return reactions, we can also form portfolios to demonstrate the robustness of the long run reversal. We implement the same strategy described above, but hold for a longer return window excluding

[9]The trading strategy holds each position for two days, leading to one trade per day for each position. Thus, we can compare the daily alpha of the trading strategy to the estimated trading costs to see if the alpha would survive adjustments for transaction costs. Following Breen et al. (2002), we estimate that a block trade of 1,000 shares for the average position in our sample would incur a cost of more than 20 basis points. This estimate coincides with the average cost from Breen et al. (2002) because the reduction in trading costs from investing only in large-cap firms in our strategy is offset by the increased trading costs associated with trading directly before the earnings announcement, consistent with So and Wang (2014).

the days immediately after announcement. Specifically, we sort stocks into portfolios based on $surprise_{t-1}$ but hold the stocks from days $t+2$ to $t+50$ after the announcement. Thus, on a given day, the portfolio is long stocks that announced earnings from 2 to 50 days ago where $surprise_{t-1}$ was below the 25th percentile of the $surprise_{t-1}$ distribution in the previous quarter and is short all stocks where $surprise_{t-1}$ was above the 75th percentile of the $surprise_{t-1}$ distribution. If there is a reversal of mispricing in the long run, we expect this trading strategy to yield negative alphas.

Column 2 of Table 5 shows that this reversal strategy yields a significant daily alpha of negative 2.6 basis points. As expected, the daily alpha for the reversal trading strategy is much smaller in absolute magnitude than the daily alpha for the direct trading strategy presented in Column 1, because the direct impact is measured over two trading days while the reversal occurs over the next 48 trading days.[10] In earlier regression analysis (see Table 4), we found that the reversal appears concentrated in the later portion of the trading window. Similarly, if we form portfolios active from $t+26$ to $t+50$, we see a slightly stronger effect, with a daily alpha of roughly negative 4 basis points. Extending the window further, from $t+51$ to $t+75$ we find no evidence of a reversal. Overall, we find evidence for both the direct impact of contrast effects as well as the reversal using calendar time asset pricing methods.

3 Alternative explanations

3.1 Information transmission

The results presented so far strongly support the contrast effects hypothesis. We now discuss potential alternative explanations involving rational, biased, and/or delayed reaction to information

[10]The direct trading strategy has an alpha of 19 bp per day for a two-day strategy, yielding a total effect of roughly 38 bp. To reverse this over the next 48 trading days necessitates -0.8 bp per day ($-\left(1.0038^{1/48}-1\right)$). The daily alpha for the reversal strategy should be roughly double this, as the direct strategy is typically active on only one of the long or short legs on a given day (holding the market on the other leg), while the reversal is typically active on both legs. Thus, we expect a daily alpha of -1.6 bp in Column 2 which is not statistically different from our point estimate of -2.6 bp (p-value of 0.44). The Column 3 reversal strategy involves 24 trading days. Using a similar argument, we expect an alpha of -3.2, which is not significantly different from our point estimate of the -4.1 (p-value of 0.66). Further, the same position can receive more or less weight in the direct and reversal strategies. The regressions in Table 4 weight each position in the reversal in the same way as in the baseline analysis and thus offer the more direct test of whether the reversal offsets the direct effect.

transmission. To focus the discussion, we will use a simple example. Suppose that firm A announces a positive earnings surprise on day $t-1$ and firm B is scheduled to announce earnings on day t. Our empirical evidence implies that following A's positive surprise, B is likely to experience low returns conditional on its actual earnings surprise. Can a story involving information transmission explain this empirical pattern?

The baseline tests have already ruled out two major classes of information transmission explanations. First, the evidence of return predictability in Columns 1 through 3 of Table 2 rules out any explanations involving rational information transmission in efficient markets. If information is released at time $t-1$ and it is quickly and properly incorporated into prices, the information should have no predictive power for future returns. As further evidence against a rational information transmission story, we show in Table 6 that A's earnings surprise does not predict B's earnings and the market does not behave as though A's announcement conveys any information relevant for B, earnings-related or otherwise. Columns 1 and 3 show that $surprise_{t-1}$ positively predicts the earnings surprises of firms scheduled to announce in the following day, but Columns 2 and 4 show this correlation goes to zero after we control for year-month fixed effects. In other words, day $t-1$ surprises do not predict day t surprises after accounting for month-level time trends. Of course A's surprise could contain non-earnings related news relevant for B. If markets are efficient, then B's stock price should change on $t-1$ when this information is first released. In Columns 5 and 6, we find no significant relation between $surprise_{t-1}$ and the $t-1$ returns of firms that are scheduled to announce the next day. The market does not behave as if A's good news conveys good or bad news on average for firm B.[11]

Second, our baseline results show that the empirical patterns cannot be explained by overre-

[11] In Internet Appendix Table 1, we also check that the results are not due to aggregation of subcases where positively or negatively correlated information is transmitted with other cases in which no information is transmitted. We look only at cases where the market reacted as if no information was transmitted in $t-1$. In this subsample, we expect to find no evidence consistent with contrast effects if the results are driven by information transmission. First, we examine only firms that announce on day t with a small open-to-open return in absolute magnitude on day $t-1$ (either below 1% or 0.5%). Within this subsample, we continue to find a strong contrast effect on day t. Next, we restrict the sample to observations for which no *negatively-correlated* information was transmitted on $t-1$ (i.e., we exclude negative return reactions to positive $surprise_{t-1}$ and positive return reactions to negative $surprise_{t-1}$). We focus on negatively-correlated information transmission because positively-correlated information predicts the opposite empirical pattern observed in the data. We again find similar results.

action to positively correlated news.[12] Using the above example, investors may mistakenly over-infer that A's positive surprise is good news for B. This over-reaction would lead to positive returns for B on day $t-1$ and then a negative return correction on average on day t when B's news is actually announced. However, A's positive surprise should not negatively affect B's *cumulative return* from $t-1$ to $t+1$ (measured starting at market close in $t-2$, before A announces) which is what we examine in Columns 4-6 of Table 2. In general, positive correlation in news implies a positive correlation between $A's$ surprise and B's cumulative returns, not the negative relation we observe in the data.

Thus, for information transmission to explain our results, investors must believe there is *negative correlation* in news, so A's positive surprise is bad news for B (e.g., A competes with B for resources). Further, investors must not fully react to this information until day t, otherwise we wouldn't observe return predictability. In general, rational investors should not react to information with a delay.[13] However, one may wonder whether a trading friction or bias other than contrast effects causes a delayed reaction. For example, boundedly rational investors may react to A's information about B with a delay because investors do not think about firm B until day t, when B becomes more salient due to news coverage surrounding its earnings announcement. We show that a delayed reaction to negatively correlated news is unlikely to explain our results for two reasons. First, we find no evidence of negative correlation in earnings news, so there is no obvious reason why investors should believe (with or without a delay) that A's positive earnings surprise is, on average, bad news for B. Second, delayed processing of information should not lead to the long-run reversals we observe in the data.

Altogether, we show that most plausible variants of the information transmission story cannot explain our results. While it is impossible to rule out all information explanations, what remains

[12]Most studies of information transmission in firm news announcements focus on the case of positive correlation in news, in which A's positive surprise is good news for B. For example, Anilowski et al. (2007) and Barth and So (2014) study "bellwether" firms whose news convey similar information for other firms.

[13]Fully rational investors in efficient markets should not react with a delay even if the interpretation of A's news for B's prospects depends on B's earnings surprise. For example, A's good news may be bad news for B, but only if B's earnings surprise is high. If investors are rational, they should realize that the average expected impact of A's positive news implies negative returns for B and react on day $t-1$.

is a very specific and complex story which must contain the following elements:

1. Investors believe that $A's\ t-1$ positive surprise contains negative information for B (contrary to the evidence in Table 6 showing that earnings surprises are positively serially correlated without accounting for time trends and not correlated after accounting for time trends).

2. The negative information relates to B's prospects other than just B's earnings.

3. Rational investors should not wait until day t to react to information released on day $t-1$. Nevertheless, the market does not react to this information until day t.

4. When the market does react to this information on day t, it reacts in a biased manner, leading to a long run reversal.

While this complex information transmission explanation is impossible to reject, we feel that the contrast effects hypothesis offers a more parsimonious explanation of the empirical results that is based on a well-known and intuitive psychological phenomenon.

3.2 Expectations vs. perceptions

A unique advantage of our financial setting is that we can identify contrast effects as an error in perceptions rather than an error in expectations. An expectational error occurs when exposure to a previous case biases expectations about the quality of the next case. For example, a gambler's fallacy is an expectational error in which, after seeing a high quality case, a judge mistakenly believes that the next case is more likely to be low quality, and this prior belief clouds the ultimate decision on the next case (Chen et al., 2016; Rabin and Vayanos, 2010). A perceptual error, such as a contrast effect, occurs if after viewing a high quality case, the judge examines the characteristics of the next case and perceives the case as lower in quality. The distinction is mainly in regard to *when* the judge makes a biased quality assessment. Under an expectational error, the judge holds mistaken beliefs about the quality of the next case before seeing the next case, whereas a perceptual error leads to a biased quality assessment only after seeing the next case. As highlighted in Chen et al. (2016), these two biases can generate observationally equivalent sequences of decision outcomes, making it difficult to distinguish between perception and expectation errors in most field settings.

Our financial setting allows us to distinguish between expectational and perceptual biases because it offers continuously traded prices. At each point in time, prices reflect current market beliefs about each firm. To see how continuously traded prices allow us to distinguish these two classes of biases, return to our example in which firm A announces a positive earnings surprise on $t-1$ and firm B announces on t. If A's announcement changes expectations about B's announcements or value, we should see B's price change on $t-1$. If these beliefs are biased, we would see a partial or full reversal on day t when B's information is revealed.[14] If A's announcement biases perceptions of B's announcements without changing expectations, B's price will not move on $t-1$, but will move in a biased manner on day t. Since we find evidence of price distortions only after B has announced (see Table 6), our evidence is consistent with a perceptual bias rather than an expectational bias.

Our focus on a perceptual bias also offers a novel contribution to the behavioral finance literature, which has largely focused on expectational biases. For example, in the context of earnings, Thomas and Zhang (2008) show an expectational bias in which the market overreacts to industry-specific news released early in an earnings season. This expectational error is corrected when a firm announces its actual earnings later in the same season. More broadly, investors form biased expectations by overextrapolating from past information (Greenwood and Shleifer, 2014; Barberis et al., 2015). These are expectational errors, as they manifest themselves upon receiving signals about a future outcome (e.g., a mistaken belief that past positive returns forecast positive future returns) rather than when the future outcome is observed (e.g., a firm's earnings announcement).[15]

Understanding how perceptual errors impact financial markets and decision-making more bro-

[14] Whether there is a full or partial reversal on day t depends on whether the news released on day t is fully revealing about B's value.

[15] Loh and Warachka (2012) document an expectational error in which investors suffer from the gambler's fallacy and expect streaks in the firm's own earnings over past quarters to reverse. Other research has shown how price responses depend on mood, sentiment, or weather (e.g., Mian and Sankaraguruswamy, 2012; Gulen and Hwang, 2012; Hirshleifer and Shumway, 2003). These return patterns may represent errors in expectations or perceptions. However, the settings usually lack the specific timing necessary to disentangle the two types of errors.

adly is important because perceptual biases can imply very different patterns than those implied by expectational biases. Expectational errors lead to mistaken predictions about future outcomes. Perceptual errors can lead to persistent mispricing even after the outcome is realized and news is revealed. As we discuss later in the Conclusion, perceptual errors may also motivate non-standard preferences such as internal and external habits, which are the basis of many influential models in macroeconomics and finance. Finally, firms can potentially exploit contrast effects as a perceptual error by timing the release of their own news to follow bad news released by other firms.[16]

3.3 Risk, trading frictions and capital constraints

Another possible concern is that the return reaction represents compensation for the impact of $surprise_{t-1}$ on risk or trading frictions. Stable firm-specific loadings on risk factors are unlikely to explain our results because we use characteristic-adjusted returns. A risk- or friction-based explanation thus requires that a more negative earnings surprise yesterday increases day-specific trading frictions or betas on risk factors, leading investors to demand a higher return as compensation.

Table 7 Panel A tests for such a channel. We modify our base specification so the return is regressed on four factors (market excess return, SMB, HML, and momentum) along with interactions of those factors with $surprise_{t-1}$. If a firm's covariation with market factors is systematically larger when there are more negative surprises on the previous day, we would expect to see large negative coefficients for the interaction terms. Examining characteristic-adjusted returns in Column 1 and raw returns in Column 2, we find no support for this hypothesis. Two coefficients are significant at the 10% level, but they are positive. None of the coefficients are significantly negative. Thus, fixed or time-varying loadings on standard risk factors are unlikely to account for our results.[17]

Another possible concern is that our findings are due to a liquidity premium. In general,

[16] Suppose a firm's news announcement is fully informative about firm value. In a simple model with expectational errors, the same strategic scheduling motives would not apply if managers only care about share price after the firm's news is released. Under an expectational error, the firm's share price would reflect the true value once the firm's news is announced, regardless of whether the announcement follows good or bad news released by another firm.

[17] In the Internet Appendix, we also check that our results are not capturing a risk premium associated with tail risk. We do not find a significant difference in the tails of return distributions split based on $surprise_{t-1}$, suggesting that the results are not due to a rational fear of extreme negative returns based on the previous day's salient surprise.

liquidity is known to be low around earnings announcements (Lee et al., 1993; So and Wang, 2014). However, for a liquidity premium to explain our results, it must be that a more negative $surprise_{t-1}$ predicts lower liquidity for firms announcing today, so that the higher return is compensation for the lower liquidity. In Table 7 Panel A Columns 3 and 4, we show that yesterday's salient surprise is not correlated with today's volume or bid-ask spread, two proxies for liquidity.

Next, we consider explanations related to limited capital. Institutional investors may heavily invest their available capital after a positive $surprise_{t-1}$ and thus have less capital at their disposal to invest the next day. If there is additional good earnings news the next day, these investors may again wish to invest, but may be constrained in their ability to do so. This could cause the price of firms announcing on day t to be lower than it would have been otherwise and result in the negative relation we previously documented. While we cannot rule out limited capital effects entirely (and indeed, we believe that limited capital can play some role in our setting), we show that the main results of the paper do not seem to be driven by limited capital among institutional investors.

We first note that limited capital is unlikely to be a major factor because even a large firm announcing on $t-1$ is small relative to the amount of liquid capital invested in US large cap stocks. Further, if a high $surprise_{t-1}$ causes investors to have limited capital to invest in a firm scheduled to announce the next day, we would expect that firm to have abnormally low trading volume and liquidity following a high $surprise_{t-1}$. We instead find that $surprise_{t-1}$ has no effect on the volume or bid-ask spread of firms scheduled to announce the following day.

If capital constraints apply to investors undertaking broad marketwide strategies, a positive $surprise_{t-1}$ should lead to lower returns for all firms on day t, not only for firms announcing earnings on day t. In untabulated results, we find that, if anything, there is a positive correlation between $surprise_{t-1}$ and the market return on day t. It could also be that $surprise_{t-1}$ is correlated with market returns on $t-1$, and it is the market performance on $t-1$ rather than $surprise_{t-1}$ that is responsible for limiting capital. Table 7 Panel B shows that the coefficient on $surprise_{t-1}$ is approximately unchanged after controlling for the market return on day $t-1$. Market returns on $t-1$ do have a marginally significant negative effect on the returns of firms scheduled to announce on day t (which could be due to capital constraints or a market-based contrast effect), but it appears to be a separate effect from that captured by $surprise_{t-1}$. In Column 4, we also find that the coefficient on $surprise_{t-1}$ remains approximately unchanged after controlling for market returns on both day $t-1$ and day t, as well as the interactions between the market returns and $surprise_{t-1}$.

Perhaps the most likely version of a capital constraints story relates to the limited capital of investors focused on earnings arbitrage. The marginal investor in firms around their earnings announcements may be funds that specialize in earnings-related trading strategies. This increases the plausibility of a limited capital explanation, as the capital need only be limited for a smaller pool of investors. In Table 7 Panel C, we examine an explanation related to limited earnings arbitrage capital by using three proxies for arbitrage capital constraints. One possibility is that arbitrage capital is more limited during market downturns. For example, the largest positive bar in Figure 4 occurs during the fourth quarter of 2008, which experienced a market return of -22%. High returns for our contrast effects trading strategy during this quarter could be due to limited capital during this period (although high returns are also consistent with the contrast effects hypothesis, as investor attention to earnings may be greater in times of greater uncertainty). Regardless, we find that excluding quarters with market returns below -5% still yields a significant contrast effect, with a slightly larger point estimate than in our baseline sample. Next, we examine the impact of VIX on our measure of contrast effects because Nagel (2012) shows that liquidity evaporates when the VIX is high. In Columns 2 and 3, we find that $surprise_{t-1}$ continues to have a significant negative effect on the return reactions of announcing firms, after controlling for VIX and the interactions of VIX with $surprise_{t-1}$. Finally, Hanson and Sunderam (2014) create a measure of the amount of arbitrage capital directed at earnings strategies. Columns 4 and 5 control for this measure and its interactions with $surprise_{t-1}$. We continue to find a significant negative coefficient on $surprise_{t-1}$. We further find no significant relation between return reactions and earnings arbitrage capital. Overall, we do not find any strong evidence that limited capital is a major driver of our results.

3.4 Strategic timing of earnings announcements

Previous research has shown that firms may advance or delay earnings announcements relative to the schedule used in the previous year or manipulate the earnings announcement itself (e.g., through adjustment of discretionary accruals). However, these types of strategic manipulation will only bias our results *if they alter firm earnings announcements as a function of the earnings surprises released by other firms on day* $t-1$. Such short-run manipulation within a single trading day is unlikely. Firms typically publicly schedule when they will announce their earnings more than a week before they actually announce (Boulland and Dessaint, 2017). The earnings surprises of other firms are, by definition, difficult to predict because they measure surprises relative to expectations. Therefore, it is unlikely that firms can strategically schedule to follow other firms with more or less positive surprises. Further, manipulation of the earnings number itself takes time and is unlikely to occur within a single day as a reaction to the earnings surprises made by other firms on day $t-1$.

To directly test strategic timing, we separately examine earnings announcements that moved or stayed the same relative to the calendar date of the announcement for the same quarter the previous year. Firms typically report their earnings on roughly the same day every year, with small changes, e.g., to announce on the same day of the week (Johnson and So, 2017). Thus, in order for strategic timing to explain our results, it must be the firms that deviate from their normal earnings announcement date that drive our results. We categorize firms as having moved their earnings date forward or backward if it differs from their previous same-quarter date by five or more days. Roughly 80% of firms keep the date the same, 10% move it forward by more than 5 days and 10% move it backwards.

We examine these sets of firms in Table 8 and find that strategic timing cannot account for the negative relation between return reactions and salient surprises at $t-1$. Firms that did not greatly move their announcement date have a large and significant negative coefficient of -0.965. Firms that moved their announcements forward or backwards have insignificant estimates of contrast effects with large standard errors. Under the strategic timing hypothesis, we should have found that firms that shifted their earnings announcement data accounted for the negative relation.

4 Robustness

This section examines whether our results are robust to alternative choices in the construction of variables. Our main analysis measures the earnings surprise as earnings relative to consensus analyst forecasts. One potential concern is that analyst forecasts may be stale or that analysts may be biased or uninformed when they make forecasts.[18] To show that our results are not caused by biases in analyst forecasts, we estimate regressions that do not utilize any analyst information. We estimate Equation 3, which does not condition on the firm's own earnings surprise, and measure the salient surprise in $t-1$ as the value-weighted return reaction for large firms that announced on day $t-1$. Our returns-based measure of the salient surprise on $t-1$ is:

$$return\ surprise_{t-1} = \frac{\sum_{i=1}^{N}\left(mkt\ cap_{i,t-4} \times return_{i,[t-2,t]}\right)}{\sum_{i=1}^{N} mkt\ cap_{i,t-4}} \tag{5}$$

In Table 9 Column 1, we find a significant coefficient of -0.052 on the new $return\ surprise_{t-1}$ measure. In Column 2, we further limit the sample to observations for which we also have analyst-based surprise measures for the firm announcing today and large firms announcing on t − 1, leading to a coefficient of -0.049. To get a sense of magnitudes, the average return responses in the lowest and highest deciles of salient return surprise are -3.6% and 3.9%, respectively. Thus, an increase from the lowest to the highest decile for $return\ surprise_{t-1}$ in Column 1 is associated with a decrease in returns of 47 basis points, similar to the 53 basis points we find in our baseline specification.

The remaining columns of Table 9 examine additional variations of the $surprise_{t-1}$ measure. In our baseline analysis, we value-weighted the earnings surprises of large firms that announced in the

[18] Despite these shortcomings, we believe that the analyst-based measure represents the most salient measure of earnings surprise. The measure is typically what is reported in the popular and financial press.

previous day to calculate $surprise_{t-1}$. This measure implicitly assumes that size is a good proxy for the relative salience of firms that announced yesterday. In Columns 3 and 4, we find similar results if we weight the firms that announced on $t-1$ using their volume or analyst coverage. In Column 5, we measure each firm's earnings surprise (and calculate $surprise_{t-1}$) using the difference between actual earnings and the median analyst forecast, without scaling by the share price. While we believe that scaling by share price is a reasonable way to compare earnings across firms with different share prices, many media outlets report earnings surprises as the unscaled difference between actual earnings and analyst forecasts, and investors may pay attention to the unscaled surprise. Using the unscaled measure, we continue to find a similar negative relation. In Column 6, we find similar results after scaling our baseline measure of $surprise_{t-1}$ by the sum of the squared size weights of each firm comprising the weighted-mean calculation. This accounts for the fact that the weighted average over a greater number of firms has a smaller standard deviation. We again find a highly significant negative relation.

In the Internet Appendix, we show the results are robust to a variety of other empirical specifications. We examine alternative size cutoffs and expanded analyst forecast windows. We also examine alternative weighting schemes, measures of returns, and date adjustments following DellaVigna and Pollet (2009). In addition, we estimate regressions controlling for day of the week, quarter of the year, firm, and/or calendar year-month fixed effects. In all specifications, we find evidence consistent with contrast effects, suggesting that our specific choices in terms of variable construction do not account for our results.

5 Further exploration of contrast effects

This section explores the specific timing and boundaries of contrast effects. In our baseline analysis, we showed how the perception of today's earnings surprise is distorted by salient surprises announced in the previous trading day. By focusing on these day-to-day sequential contrast effects, we were able to rule out many potential alternative explanations involving information transmission or expectational errors by looking at the timing of when prices changed. Since earnings announcements are scheduled several weeks in advance, we were also able to rule out strategic timing explanations by looking at contrast effects across sequential trading days. However, contrast effects do not necessarily need to manifest across sequential trading days. In theory, contrast effects could occur sequentially within the same day or even contemporaneously. Contrast effects could also occur across lags of longer than a single day and investors may even revise their perceptions of the current earnings announcement after observing salient earnings announcements in the future.

Previous empirical tests of contrast effects in laboratory or non-financial settings have shown that individuals react strongly only to recent observations and do not revise their perceptions of the current case in light of future signals. For example, Bhargava and Fisman (2014)'s study of speed dating finds that the appearance of the person whom you spoke with most recently has the largest impact on the current dating decision (see also Damisch et al., 2006 and Kenrick and Gutierres, 1980). However, it is an open question whether the same recency predictions will apply to investor perceptions of earnings and what time interval investors will consider to be "recent." Finally contrast effects could be stronger if the previously viewed signal is more salient or believed to be more relevant or comparable to the current signal. In this section, we explore the timing of how contrast effects manifest themselves in financial markets and additional heterogeneity effects.

5.1 Lead and lag effects across days

We begin by exploring whether further lags of salient surprises released on days $t-2$ and $t-3$ impact the perception of today's earnings surprise. The first column of Table 10 Panel A regresses the return reaction to today's announcement on $surprise_{t-1}$ as well as further lags of surprises on $t-2$ and $t-3$. To ensure that our return measure allows for a response to information covering the entire time period (see Section 3.1), we examine the return from $t-3$ to $t+1$ as the dependent variable. We find a strong and significant negative relation between the previous day's salient surprise and the return response to firms announcing today. Meanwhile, we find a smaller, insignificant, and inconsistently signed relation between returns and earlier surprises on $t-3$ and $t-2$. We can reject that the return reaction to $t-1$ surprises is equal to the reactions to $t-2$ or $t-3$ surprises with a p-value below 0.01. These results suggest that investors react more to recent salient surprises than those further in the past, although we do not rule out the possibility of a small lagged effect from $t-2$ or $t-3$, which we lack the power to estimate precisely.

Next, we examine how return reactions to firms announcing today are affected by future surprises announced on days $t+1$ and $t+2$. We use returns from $t-1$ to $t+3$ as our dependent variable, to allow for the return reaction of a firm that announces on day t to respond to these future earnings announcements. Such a response would require that investors revise their initial perceptions of day t announcements in light of subsequent earnings announcements released in the following two days. In Column 3 of Table 10 Panel A, we find the relations between return responses and salient surprises on days $t+1$ and $t+2$ are small, vary in sign, and are insignificant. This suggests that investors do not significantly revise their perceptions of earnings announcements in light of subsequent announcements, although we again do not reject the possibility of small effects given that we cannot estimate precise zero coefficients.

One possible concern is that the longer lags and leads are more likely to extend over a weekend, and the weekend may impact how contrast effects manifest. In Columns 2 and 4, we repeat the analysis after limiting the sample to observations in which today's announcement occurs on a Thursday and Friday or Monday and Tuesday, such that the lag and lead measures of salient surprises released by other firms occurred within the same week without a weekend break. The results remain materially similar.

Almost any empirical exercise involves the worry that there is a mechanical relation due to specification choice. In addition to providing suggestive evidence of the transitory nature of contrast effects, these results offer a placebo test for this concern. If the negative coefficient on $surprise_{t-1}$ is mechanically due to our specification, then the coefficients on $t-2$ or $t+1$ should be similarly biased. Given that we do not find such a relation, we feel confident that our empirical choices are not mechanically driving the results.

5.2 Within-day contrast effects

The analysis so far has examined contrast effects across trading days. We can also examine contrast effects within the same day. We present the following analysis as supplementary to our baseline estimates because data on the within-day timing of earnings announcements is only available for a subset of announcements and for years after 1995. Further, some firms do not preschedule the time of announcement even though they do pre-commit to the date of announcement.[19] Nevertheless, we can explore whether the data is consistent with contrast effects occurring within the same day.

We categorize firms as announcing in the morning (before market open at 9:30 am) or afternoon (after market close at 4:00pm).[20] We then test whether perceptions of the afternoon announcements are biased by the salient morning announcements and vice versa. We also test for potential contemporaneous contrast effects induced by announcements in the same half or full day period. Overall, we find that contrast effects from earlier morning salient announcements significantly bias return reactions to announcements made later in the afternoon.

We also do not rule out the possibility of contrast effects from other announcements in the same half-day period or later in the day, and indeed find some suggestive evidence that these effects may exist. However, we lack the statistical power to estimate these effects precisely, because we only observe time stamps for a limited set of announcements. Further, our measures of the salient surprises released by other firms in each half-day window suffer from noise, because there may be other large firms that announced salient surprises in the same half-day window, but which cannot be identified, because their announcements lack time stamps. Finally, investors may become aware of earnings announcements in a different sequential ordering than the ordering in which the earnings are actually announced.

To test within-day contrast effects, we estimate Equation 4, but with three modifications. First, for each day t, we calculate two half-day salient surprises: the surprise of large firms that announced before market open ($AM\ surprise_t$) and the surprise of large firms that announced after market

[19] Bagnoli et al. (2005) present evidence that firms with bad news strategically choose to announce in the afternoon.
[20] We exclude firms announcing in the interim time period (roughly 8% of the value-weighted average of firms with known announcement times).

closure ($PM\ surprise_t$). Second, when we test for contemporaneous contrast effects, we exclude the current announcing firm from the calculation of the salient surprise, so that the salient surprise refers only to the announcements of other firms. Third, for our return measure, we examine returns from the close on $t-1$ to the close on $t+1$, as this window includes both the response to the morning or afternoon surprises of other firms as well as the response to the firm's own announcement.

We begin by examining whether return reactions to firms announcing on day t are distorted by the announcements of other firms that also announce on day t, regardless of the timing within each day. In Table 10 Panel B Column 1, we estimate a negative coefficient on the same-day measure of salient surprise of -0.587 that is significant at the 10 percent level. This result is consistent with contrast effects operating approximately contemporaneously among earnings announcements made in the same day. However, we caution that any evidence of "contemporaneous" contrast effects may be driven by investors who actually observed news coverage of the earnings announcements sequentially within a given time interval rather than simultaneously. Next, we show in Column 2 that return reactions to afternoon announcements are inversely affected by $AM\ surprise_t$. This same-day measure of sequential contrast effects is slightly larger than the across-day measures estimated in earlier tables, consistent with more recent observations leading to larger contrast effects. We also explore whether salient PM surprises have a negative impact on the return reaction to AM announcements. Note, the return window (which extends to $t+1$) does not preclude such an effect, as investors could revise their response to AM announcements due to announcements released in the PM. In Column 5, we find a negative but noisy coefficient on the $PM\ surprise_t$. Thus, we do not reject the possibility that perceptions of AM announcements are inversely affected by announcements made later in the day, although we lack the power to estimate such effects precisely.

Finally, we test whether return reactions to earnings announcements in each half day interval are inversely affected by other salient surprises in the same half day interval, or previous half-day interval (including the most recent half day on the previous trading day). The results in Columns 3, 4, and 6 suggest that we lack the statistical power to strongly conclude in favor or against the existence of such effects. In Table 5 in the Internet Appendix, we also test whether return reactions to announcements in the AM or PM on day t are biased by salient surprises released by other firms with known time stamps in the current and up-to-four lagged half day intervals (including those on days $t-1$ and $t-2$). Overall, we find significant evidence of return reactions to PM announcements being distorted by AM announcement in the same day. We also continue to find evidence that return reactions to today's announcements (AM or PM) are inversely affected by salient surprises released in the previous trading day, particularly those released in the morning on $t-1$. While the difference is insignificant, the stronger effect for $surprise_{t-1}\,AM$ than for $surprise_{t-1}\,PM$ is consistent with the fact that larger firms tend to announce in the morning, so morning announcements may be more salient to investors.

5.3 Day of the week and quarter of the year

$Surprise_{t-1}$ typically occurs on the previous calendar day, except when the current announcement occurs on a Monday. The salience of $surprise_{t-1}$ may decay over the weekend, leading to less of a contrast effect when the current earnings surprise is announced on a Monday. Alternatively, the salience of $surprise_{t-1}$ may increase over the weekend, perhaps because investors have more time to think about Friday announcements. Finally, it could be that ordering is the only aspect of timing that matters for attention (as in classic studies of recency, e.g., Murdock Jr, 1962), in which case, contrast effects on Mondays will be similar to that of other days. In Table 10 Panel C, we examine contrast effects separately for announcements on each of the five days of the week. While we lose significance once we divide our main sample into five subsamples, we find negative coefficients on $surprise_{t-1}$ for each of the five days of the week, including Monday. We estimate the largest contrast effect when the current announcement occurs on a Friday, although the Friday sample also includes the smallest number of observations, and we are not able to reject the null hypothesis that contrast effects are equal across the five days. The estimates suggest that our baseline results are not driven by unusual behavior on any particular day of the week.

One may also be concerned that earnings surprises differ systematically by day of the week. For example, Penman (1987), Damodaran (1989), and Bagnoli et al. (2005) find that earnings surprises are slightly higher on non-Fridays and lower on Fridays. We can rule out that our results are driven by simple differences in mean responses across days by controlling for day of the week fixed effects (the results are reported in the Internet Appendix, Table 4). In addition to differences in means, there may be differences in the variation around these fixed effects, especially with respect to Friday announcements. To address this concern, we only examine how return reactions to earnings announcements on Tuesday, Wednesday and Thursday are affected by $surprise_{t-1}$ (the results are reported in Table 10 Panel C). We find a significant coefficient of -0.686, which is slightly smaller than but insignificantly different from our baseline estimate. Thus, systematic differences in means and variances in earnings surprises across days of the week are unlikely to explain our findings.

We also explore how the magnitude of contrast effects varies by quarter of the year in Panel D. We find negative coefficients on $surprise_{t-1}$ for all four quarters, with the largest absolute magnitudes in Q1. The larger effect in Q1 could potentially be caused by investors paying more attention to fiscal year end earnings announcements, which are concentrated in calendar year Q1. While suggestive, we are not able to reject the null hypothesis that contrast effects are equal across the four quarters. However, the estimates suggest that our baseline results are not driven by unusual behavior in any particular quarter.

5.4 Heterogeneity

In our baseline analysis, we focus on large firms both in the measurement of yesterday's salient surprise and the weighting of observations for firms announcing earnings today. In Table 11, we explore how the magnitude of the contrast effect varies with the size of the firm announcing earnings today. The first column breaks the coefficients down by size quintile of the firm releasing earnings on day t. We find that the smaller quintiles have the expected negative coefficients, but these coefficients are smaller in magnitude and sometimes insignificant, while the largest (fifth) quintile is driving the results. These results show that our early findings are not driven only by small firms as is the case with many other asset pricing anomalies.

However, these results do not prove that contrast effects are weak for small firms. Rather, we could measure strong contrast effects for large firms announcing today because investors tend to contrast large firms releasing earnings today with other large firms that released earnings yesterday. Investors of smaller firms may contrast the earnings of small firms with that of other similar firms that released earnings yesterday. However, because multiple firms release earnings on $t-1$, it is difficult for us, as econometricians, to identify which firms are salient to investors for each small firm announcing earnings today. This is a point that we explore in detail in Section 5.5, where we show that contrast effects are sizable and significant for smaller firms once we look within industries.

The second column explores heterogeneity in the number of analysts covering firms that release earnings today. In general, the more interest the market has in a given firm, the more analysts will cover that firm's earnings announcement. We examine contrast effects separately for firms covered by one, two, and three or more analysts. We find that contrast effects are driven by observations in which the current announcing firm has analyst coverage of two or more. This shows that our findings are not driven by small firms with little analyst coverage. However, we again caution that these results do not imply that investors in firms with little analyst coverage do not suffer from contrast effects. Rather, these investors may contrast these smaller, niche firms with a specific set of other similar small firms that we have difficulty identifying.[21]

Finally, we explore how our results vary over time. We find evidence consistent with contrast effects in each decade of our sample: we estimate a contrast effect of -0.663 in the 1980s, -1.024 in the 1990s, -0.542 in the 2000s, and -1.001 after 2010. The large and significant estimate of contrast effects in the 2010s shows that our results are unlikely to be driven by date recording errors in the early period in I/B/E/S (we present additional tests exploring potential date recording errors in the Internet Appendix). It may also seem puzzling that the magnitude of the contrast effect remains large in the 2010s when the costs of conducting arbitrage are likely to have declined. However, we note that arbitrageurs may not have been aware of the mispricing induced by contrast effects prior

[21] We face the additional measurement challenge that the earnings surprises of small firms are measured with greater error because our measure of market expectations is likely to be noisier due to reduced analyst coverage. This implies that we may control for the actual earnings surprises of small firms with more error.

to the circulation of this paper. Further, financial media coverage of earnings seasons has increased over time, which may have made the earnings surprises released by other firms more salient in the minds of investors, thereby exacerbating the contrast effects bias.

5.5 Industry contrast effects

As discussed in the previous section, while we measure stronger contrast effects among larger firms, contrast effects could also affect the returns of smaller firms. Investors may compare smaller firms to a subset of similar firms that announced in the previous day. If so, our baseline empirical specification will underestimate the true magnitude of contrast effects for small firms announcing on day t because we measure the salient surprise in $t-1$ as the value-weighted average of earnings surprises among all large firms that announced in $t-1$.

It is difficult to know what the right comparison group is for any firm, but one reasonable possibility is other firms in the same industry. In this section, we explore how contrast effects depend on whether the firms announcing today and yesterday belong to the same industry. We find that contrast effects for large firms can be strong both within and across industries. However, across-industry contrast effects are only strong if there is no same-industry comparison firm available. If the previous day had announcements from large firms in both the same and different industries, we find a larger effect for the same-industry announcement. In addition, for smaller firms announcing today, we find that contrast effects primarily operate through within-industry comparisons.

In Table 12, we modify our baseline specification to include two measures of $surprise_{t-1}$: one based on other firms announcing in the same industry as the firm announcing on day t and one based on other firms in different industries. To form these two salient surprise measures, we continue to use the value-weighted average surprises of firms above the 90th percentile of market capitalization, under the assumption that, even within industry, larger firms are more likely to be more salient.[22] We present results using the very broad Fama French 5 industry classification, because a limited set of firms announce earnings on $t-1$, and if we use narrower industry definitions, we often lack

[22]In untabulated results, we find a similar pattern if we expand the definition of salient surprise to allow for the inclusion of smaller firms that announced on $t-1$.

another firm announcing within the same industry. We also caution that companies may be related in a variety of ways that matter to investors, and these relations will be imperfectly captured by any industry classification system. Thus, the results are based on a noisy proxy of what investors are paying attention to.

A limited number of large firms (median of 6) announce earnings on $t-1$, and there are usually fewer firms in the same industry as the firm announcing on day t than firms in different industries. This implies that the standard deviation of the different-industry salient surprise will be relatively smaller, as the average of a larger sample has a smaller standard deviation. To make the magnitudes of the coefficients on the $t-1$ salient surprises in the same- and different-industry samples comparable, we scale each salient surprise by the sum of the squared size weights of each firm comprising the weighted-mean calculation. While this scaling makes the coefficients for the same and different industry salient surprises comparable to one another, the magnitude of these coefficients should not be compared to those in other tables (with the exception of Table 9 Column 6). In addition, if no firm announced within the same (different) industry on $t-1$, we set the relevant $surprise_{t-1}$ variable to zero and include a dummy variable equal to one when the same (different) industry $surprise_{t-1}$ is missing.

Table 12 modifies our baseline specification to use the two separate measures of salient surprise on day $t-1$. Column 1 is value-weighted by the market capitalization of the firm announcing earnings today while Column 2 is equal-weighted. Thus, Column 1 overweights larger firms relative to Column 2. We find that, when large firms are weighted more heavily, the magnitude of the contrast effect is similar within and across industries. When smaller firms are weighted more heavily as in Column 2, the contrast effect is more than twice as large within the same industry. In Column 3, we again value-weight the regression to focus on large firms, but include only days where both same industry and different industry $surprise_{t-1}$ are not missing. On such days, large firms exhibit a stronger and more significant contrast effect in response to firms in the same industry. However, the same and different industry coefficients are not statistically different from one another.

The final four columns separately examine the sample of small firms (below median market capitalization in each year) and large firms (above the median in each year) announcing on day t. We find that small firms exhibit stronger contrast effects with same industry firms than with different industry firms, particularly when both same and different industry firms announced earnings in the previous trading day. Large firms tend to be contrasted with other large firms regardless of industry. However, if there was a same industry announcement in the previous trading day, the contrast effect within the same industry dominates that of different industry. Again, the differences are not statistically significant, as indicated by the p-values at the bottom of the table.

Overall, these results are consistent with a world in which investors in smaller firms pay more attention to previous announcements by other firms in the same industry. Meanwhile, investors in larger firms pay attention to the recent earnings announcements of other large firms, but pay relatively more attention to same-industry announcements if such a comparison is available. This suggests that the magnitude of contrast effects depend on whether agents consider signals to belong to the same category. In this paper, we have shown that industry and size affect relative comparisons among earnings announcements. We leave the important question of how the boundaries of comparison sets are formed more generally for future research.

6 Conclusion

We present evidence of contrast effects in sophisticated financial markets: investors mistakenly perceive information from earnings announcements in contrast to what preceded it. The scheduling of when earnings are announced is usually set several weeks before the announcement, so whether a given firm announces following positive or negative surprises by other firms is unlikely to be correlated with the firm's fundamentals. We find that the return reaction to an earnings announcement is inversely related to the level of earnings surprise announced by large firms in the previous day. This implies that market prices react to the relative content of news instead of only reacting to the absolute content of news.

The existing empirical literature on contrast effects mainly comes from laboratory settings, and the limited field evidence focuses on households making infrequent dating or real estate decisions. Our results show that contrast effects impact equilibrium prices and capital allocation in sophisticated markets with professionals making repeated investment decisions. In our financial setting, all investors are likely to have observed similar earnings news prior to interpreting the current earnings surprise. Thus, investor perceptions will generally be biased in the same direction based on the common salient event. If investors instead each viewed different events, some investors might be positively biased while others are negatively biased, leading to no strong net impact on market prices. A variety of other market news events are also likely to be viewed by investors as a common sequence and thus lead to contrast effects distorting market prices. Possible examples include recent market performance, macroeconomic news announcements, a firm's own previous earnings announcements, or non-earnings related firm announcements. We leave the exploration of whether contrast effects also apply to perceptions of these other types of financial events for future research.

Our results also suggest that contrast effects have the potential to bias a wide variety of important real-world decisions outside of financial markets, including judicial sentencing, hiring and promotion decisions, firm project choice, and household purchase decisions. Within financial markets, we find that the mispricing induced by contrast effects reverses within approximately 50 trading days. Such corrections are less likely to occur in other non-traded settings such as hiring decisions or firm project choice. Thus, contrast effects can potentially lead to even more costly mistakes in non-financial settings.

While we focus on showing that contrast effects bias perceptions of news, contrast effects may also provide a psychological basis for non-standard *preferences*, such as internal habit formation, that are the basis of many influential models in macroeconomics and finance. Under internal habit formation, individuals value gains in consumption relative to previous experience rather than its absolute level. These preferences could arise because past high levels of consumption lead individuals to perceive any amount of current consumption as lesser in comparison. Similarly, a large literature has studied external habits and the role of relative earnings or "keeping up with the Joneses" preferences. This literature has generally assumed that individuals are motivated by feelings of envy and jealousy, but contrast effects may be another important contributing factor. Even if individuals do not directly desire to consume as much or more than their peers, contrast effects may lead individuals to perceive their own consumption as lesser than its true level if the high consumption of peers is very salient.

Finally, to attain a clean measure of contrast effects, we chose a financial setting in which firms cannot strategically use contrast effects to their advantage because they pre-commit to when they will announce earnings. However, in other settings, agents with discretion over the timing of information disclosure may schedule the release of news in order to take advantage of contrast effects bias. For example, a firm with very bad news to release may try to release that news after another firm releases bad news, so that the perception of its own news is not as negative. Such strategic manipulation of market biases may be a promising direction for future research.

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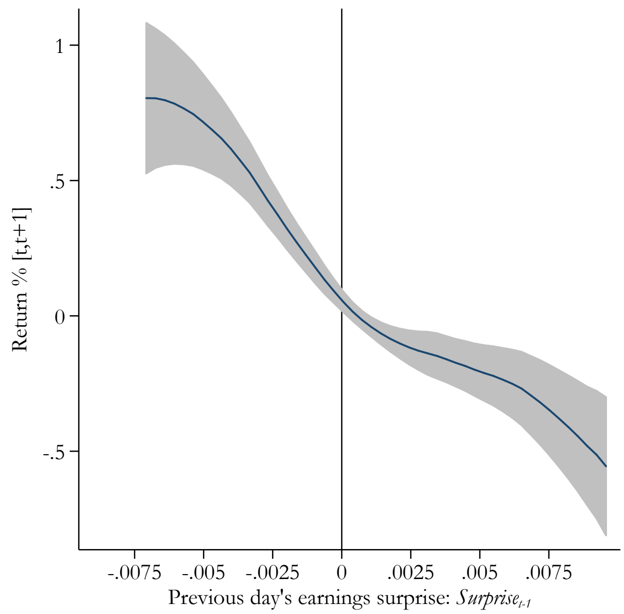
Figure 2
Return Reaction to Earnings $Surprise_{t-1}$

This graph shows the relation between return reactions to firm earnings announcements on day t and the salient surprise ($surprise_{t-1}$) announced by other firms on day $t-1$ (calculated as the value-weighted earnings surprises of large firms that announced earnings on day $t-1$), estimated using a value-weighted local linear regression with the epanechnikov kernel and the rule-of-thumb bandwidth (Silverman, 1986). We define a "large" firm as a firm with market capitalization at $t-4$ exceeding the 90th percentile cutoff of the NYSE index in that month. Gray areas indicate 90 percent confidence intervals. Panel A reports unconditional returns without controlling for the firm's own earnings surprise, demeaned by the value-weighted average return in the sample. The return is the open-to-open return measured over the interval $[t, t+1]$, from market open on t to market open on $t+2$. Panel B reports return residuals after controlling for 20 bins in terms of the firm's own earnings surprise. The return is measured over the interval $[t-1, t+1]$, the period from market close on $t-2$ to market close on $t+1$.

Panel A: Unconditional Predictive Relation



Panel B: Conditional on Own Earnings Surprise

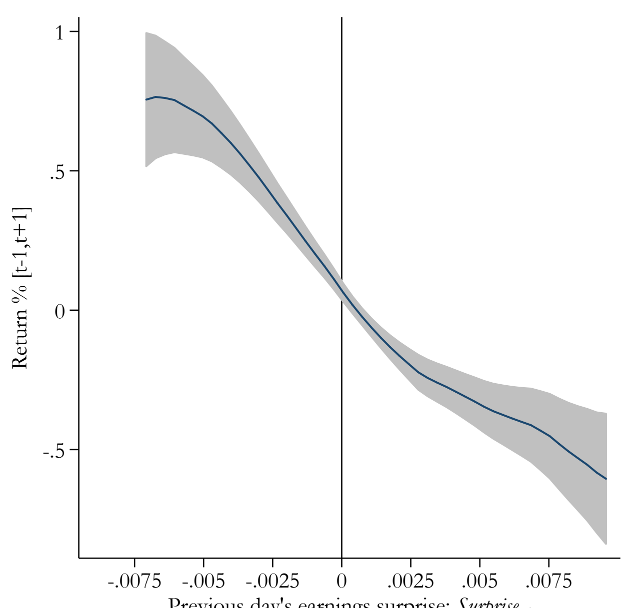


Figure 3
Return Reaction to Own Earnings Surprise

This graph shows the returns of firms that announced earnings on day t against the percentile ranks of the firm's own earnings surprise, estimated using a value-weighted local linear regression with the epanechnikov kernel and the rule-of-thumb bandwidth (Silverman, 1986). The graph shows two subsamples: return reactions following $surprise_{t-1}$ in either the lowest or highest deciles. Gray areas indicate 90 percent confidence intervals. The vertical line indicates zero earnings surprise.

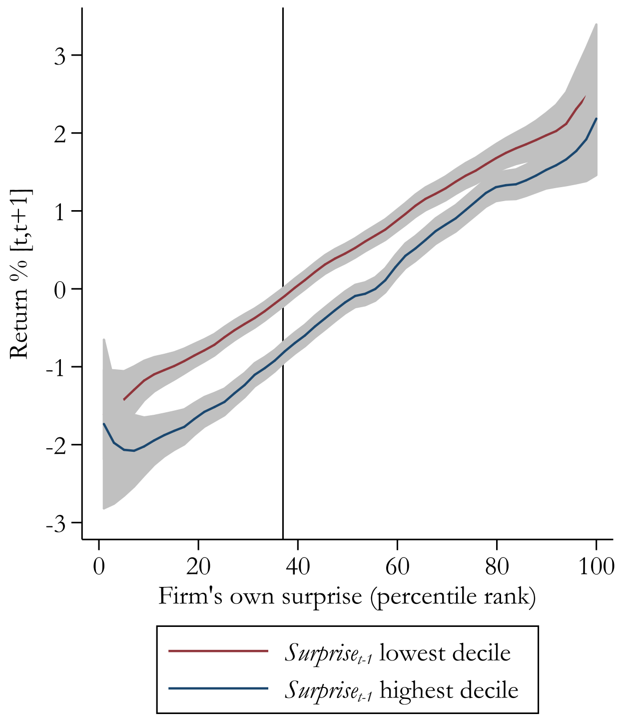


Figure 4
Distribution of Returns by $\boldsymbol{Surprise_{t-1}}$

This graph shows the average daily return (in %) of the trading strategy described in Table 5 Column 1 based on positions held on days t and $t+1$.

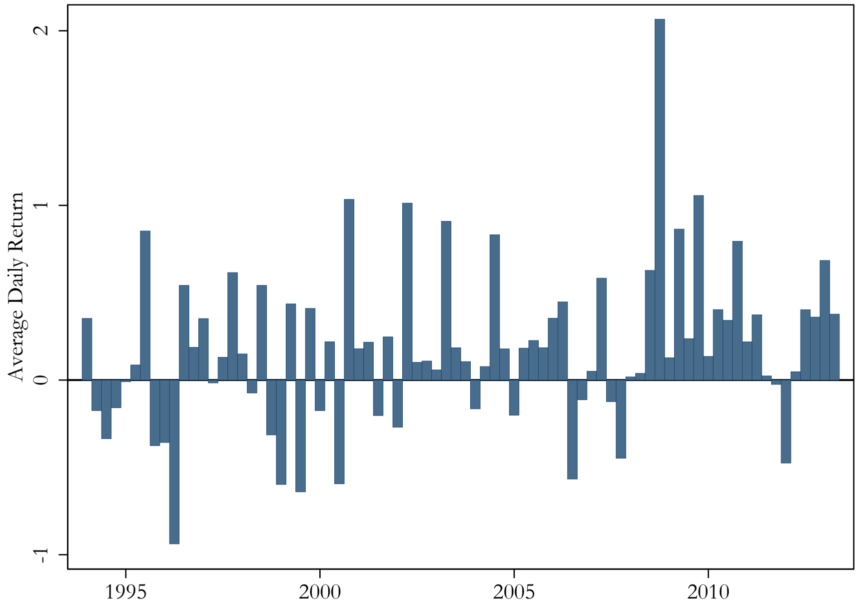


Table 1
Summary Statistics

This table presents summary statistics for the main variables used in our analysis using data from 1984 to 2013. The earnings surprise is measured as $(actual - forecast)/price_{t-3}$ where $forecast$ is the median of each analyst's most recent forecast that is released within 15 days of the announcement, excluding t and $t-1$. Returns are the return of a firm minus the return of a portfolio matched on quintiles of market capitalization, book-to-market ratio, and momentum (excluding firms used in the calculation of $surprise_{t-1}$ and the announcing firm). $Surprise_{t-1}$ is our baseline measure of the salient surprise released by other firms in the previous trading day. It is calculated as the value-weighted earnings surprise of all large firms that announced in the previous trading day. We define a "large" firm as a firm with market capitalization three days before its earnings is announced that exceeds the 90th percentile cutoff of the NYSE index in that month.

	N	Mean	SD	P25	P50	P75
Own earnings surprise	75897	-0.0003	0.0137	-0.0003	0.0002	0.0015
Return [t-1, t+1]	75897	0.0018	0.0701	-0.0315	0.0007	0.0351
Return [t, t+1] (open-to-open)	61640	0.0009	0.0693	-0.0314	0.0004	0.0340
Market Cap$_{t-3}$ ($M)	75897	7677	24100	440	1487	5057
Number of analysts [t-15, t-2]	75897	3.722	3.671	1	2	5
Surprise $_{t-1}$	75897	0.0005	0.0017	0.0000	0.0004	0.0010
Number of surprises [t-1], large firms	75897	7.578	5.800	3	6	12

Table 2
Baseline Results

This table explores the relation between return reactions for firms that announce earnings today and the earnings surprises of other firms that announced in the previous trading day. Columns 1-3 measure returns for announcing firms from market open on day t to market open on day $t+2$ while Columns 4-6 examine returns from market close on $t-2$ to market close on $t+1$. This return for announcing firms is regressed on various measures of the salient earnings surprise from $t-1$. Returns are the return of a firm minus the return of a portfolio matched on quintiles of market capitalization, book-to-market ratio, and momentum (excluding firms used in the calculation of $surprise_{t-1}$ and the announcing firm). Surprises for the firms announcing today and in the previous trading day are measured as $(actual - forecast)/price_{t-3}$ where $forecast$ is the median of each analyst's most recent forecast that is released within 15 days of the announcement, excluding t and $t-1$. We define a "large" firm as a firm with market capitalization three days before its earnings is announced that exceeds the 90th percentile cutoff of the NYSE index in that month. Columns 1 and 4 measure $surprise_{t-1}$ as the earnings surprise of the largest firm (conditional on it being a large firm) the announced in the previous trading day. Columns 2 and 5 measure $surprise_{t-1}$ using the equal-weighted earnings surprise of all large firms that announced in the previous trading day. Columns 3 and 6 measure $surprise_{t-1}$ as the value-weighted earnings surprise of all large firms that announced in the previous trading day. Columns 4-6 include controls for 20 equally sized bins in terms of the earnings surprise of the firm that announced today, plus a dummy for zero earnings surprise. We refer to Column 6 as our baseline specification in later tables. Observations are value-weighted by the $t-3$ scaled market capitalization of the firm announcing earnings today. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Open-to-open return $[t, t+1]$			Close-to-close return $[t-1, t+1]$		
	(1)	(2)	(3)	(4)	(5)	(6)
$Surprise_{t-1}$ of largest firm	-0.526***			-0.608***		
	(0.199)			(0.178)		
$Surprise_{t-1}$ large firms, EW mean		-0.846***			-1.041***	
		(0.289)			(0.254)	
$Surprise_{t-1}$ large firms, VW mean			-0.780***			-0.924***
			(0.240)			(0.229)
Own $surprise_{it}$ controls	No	No	No	Yes	Yes	Yes
R^2	0.000584	0.000753	0.000841	0.0596	0.0600	0.0600
Observations	61640	61640	61640	75897	75897	75897

Table 3
Potential Interaction Effects

This table examines whether contrast effects are related to an interaction between $surprise_{t-1}$ and the firm's announced surprise on day t. Column 1 measures the surprise today using the level, Column 2 measures it using 20 equally sized bins, and Column 3 uses quintiles. For brevity, we report only the interaction effects, but all direct effects are included in the regressions. All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Return $[t-1, t+1]$		
	(1)	(2)	(3)
$Surprise_{t-1}$	-0.703***	-1.468***	-1.442**
	(0.227)	(0.529)	(0.722)
$Surprise_{t-1}$ x own surprise	14.56		
	(40.05)		
$Surprise_{t-1}$ x own surprise (20 bins)		0.0581	
		(0.0497)	
$Surprise_{t-1}$ x own surprise quintile 2			0.257
			(0.909)
$Surprise_{t-1}$ x own surprise quintile 3			0.682
			(0.929)
$Surprise_{t-1}$ x own surprise quintile 4			0.844
			(0.831)
$Surprise_{t-1}$ x own surprise quintile 5			0.893
			(1.082)
R^2	0.0123	0.0555	0.0562
Observations	75897	75897	75897

Table 4
Long Run Reversals

This table examines the relation between $surprise_{t-1}$ and long run return reactions. Return windows are as indicated in column headers. All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Return window:	$[-1, +1]$	$[-1, +25]$	$[-1, +50]$	$[-1, +75]$	$[+2, +25]$	$[+26, +50]$	$[+51, +75]$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Surprise_{t-1}$	-0.924***	-0.860*	0.365	0.100	0.112	1.152**	-0.316
	(0.229)	(0.515)	(0.686)	(0.776)	(0.431)	(0.475)	(0.522)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0600	0.0222	0.0115	0.00862	0.00189	0.00236	0.00270
Observations	75897	75428	74122	73478	75428	74122	73478

Table 5
Trading Strategy

This table examines the returns of a contrast effects trading strategy. For the strategy in Column 1, on days where $surprise_{t-1}$ is below the 25th percentile of $surprise_{t-1}$ over the previous quarter, we long stocks with an earnings announcement on day t. On days where $surprise_{t-1}$ is above the 25th percentile of $surprise_{t-1}$ over the previous quarter, we short stocks with an earnings announcement on day t. The position is held for days t to $t+1$ beginning at market open on day t. If this strategy is only active in the long (short) leg on a given day, we short (long) the market. The returns correspond to a strategy that trades directly on the short-term contrast effect. In Columns 2, 3 and 4, the position is held for days $t+2$ to $t+50$, $t+26$ to $t+50$ and $t+51$ to $t+75$, respectively, using close-to-close returns. These returns correspond to strategies that trade on potential reversals. For example, in Column 2, on a given day, the portfolio is long stocks that announced earnings from 2 to 50 days ago where $surprise_{t-1}$ was below the 25th percentile of the $surprise_{t-1}$ distribution in the previous quarter and is short all stocks where $surprise_{t-1}$ was above the 75th percentile of the $surprise_{t-1}$ distribution. For all strategies, we include only stocks with a market capitalization above the 80th percentile of the NYSE. Each portfolio is value-weighted based on market capitalization on $t-3$. We compute abnormal returns by regressing daily portfolio returns on the market, SMB, HML, UMD, and short term reversal risk factors. Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	$[t, t+1]$	$[t+2, t+50]$	$[t+26, t+50]$	$[t+51, t+75]$
	(1)	(2)	(3)	(4)
Alpha [%]	0.189***	-0.0257**	-0.0407**	-0.0104
	(0.0558)	(0.0127)	(0.0196)	(0.0226)
Mkt	-0.0371	0.0311***	0.0415**	0.0462**
	(0.0461)	(0.0115)	(0.0180)	(0.0206)
SMB	0.0693	0.0238	-0.00250	-0.0381
	(0.0871)	(0.0216)	(0.0333)	(0.0382)
HML	-0.182**	-0.0107	-0.0700**	0.0222
	(0.0825)	(0.0222)	(0.0340)	(0.0390)
UMD	0.000897	0.00143	0.0336	-0.0846***
	(0.0597)	(0.0147)	(0.0227)	(0.0261)
ST Reversal	-0.0909	0.000462***	0.000834***	-0.000517**
	(0.0716)	(0.000149)	(0.000230)	(0.000263)
Observations	1525	5064	4781	4773

Table 6
Information Transmission

This table examines whether $surprise_{t-1}$ predicts earnings surprises on day t or conveys other information relevant for stocks with earnings announcements scheduled on day t. The dependent variable in Columns 1 and 2 is the surprise of the firm that announces on day t. The dependent variable in Columns 3 and 4 is the bin (1 through 20, equally sized) for the surprise of the firm that announces on day t. Columns 2 and 4 include year-month fixed effects. Columns 5 and 6 explore the $t-1$ return reaction of the firm scheduled to announce on day t to $surprise_{t-1}$. The dependent variable is the $t-1$ return for the firm scheduled to announce on day t, measured as close-to-close returns in Columns 5 and open-to-open returns in Columns 6. All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	$Surprise_{it}$		20 $surprise_{it}$ bins		Close-to-close ret $[t-1]$	Open-to-open ret $[t-1]$
	(1)	(2)	(3)	(4)	(5)	(6)
$Surprise_{t-1}$	0.153***	0.00660	134.2***	-30.33	0.0366	0.117
	(0.0565)	(0.0563)	(32.86)	(27.47)	(0.135)	(0.161)
Year-month FE	No	Yes	No	Yes	No	No
R^2	0.00194	0.0325	0.00286	0.0649	0.0000122	0.000125
Observations	75897	75897	75897	75897	75897	61640

Table 7
Risk, Trading Frictions, and Limited Capital

This table tests whether the negative relation between return reactions and $surprise_{t-1}$ is driven by changes in risk or trading frictions. Panel A Columns 1 and 2 test whether the negative relation is driven by changes in risk, as measured by the betas of the market, SMB, HML, and UMD risk factors. We regress our baseline return measure (Column 1) or the raw return (Column 2) on the four factors, $surprise_{t-1}$, and the interaction between $surprise_{t-1}$ and the four factors. Columns 3 and 4 test whether the negative relation is driven by changes in liquidity, measured as the log of daily dollar volume in Column 3 and the log of the bid-ask spread in Column 4. Measures of liquidity vary greatly across firms, so Columns 3 and 4 include firm fixed effects. Panel B examines how return reactions to firm announcements on day t vary with market performance on $t-1$ and t as well as interactions of $surprise_{t-1}$ with each of these measures. Panel C examines how contrast effects vary with various proxies for limited capital. Column 1 excludes quarters with a market return below -5%. Column 2 includes a control for the VIX and Column 3 adds an interaction between the VIX and $surprise_{t-1}$. Column 4 and 5 examine contrast effects after controlling for the Hanson and Sunderam (2014) measure of arbitrage capital directed at earnings strategies as well as its interaction with $surprise_{t-1}$. Note that the Hanson and Sunderam (2014) measure is based on standardized unexplained earnings (SUE) rather than the earnings surprise relative to analyst forecasts, as used in this paper. All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Risk and Liquidity

	Return $[t-1, t+1]$	Raw ret $[t-1, t+1]$	Log(volume)	Log(bid-ask)
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$	-0.975***	-1.149***	2.717	1.634
	(0.211)	(0.221)	(4.965)	(5.513)
Mkt-rf x $surprise_{t-1}$	5.027	5.149		
	(8.949)	(8.692)		
SMB x $surprise_{t-1}$	-9.881	-10.40		
	(20.27)	(22.88)		
HML x $surprise_{t-1}$	25.44	30.12		
	(25.35)	(26.35)		
UMD x $surprise_{t-1}$	27.74*	43.43***		
	(14.71)	(14.35)		
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
R^2	0.0616	0.196	0.891	0.754
Observations	75897	76062	75734	68750

Panel B: Potential Market Effects

	Return $[t-1, t+1]$			
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$	-0.923***		-0.921***	-0.965***
	(0.233)		(0.231)	(0.226)
$Market_{t-1}$	-0.0641*	-0.0644*	-0.0748*	-0.0721*
	(0.0369)	(0.0357)	(0.0420)	(0.0421)
$Surprise_{t-1}$ x $market_{t-1}$			6.141	7.882
			(12.32)	(12.39)
$Market_t$				0.0457
				(0.0500)
$Surprise_{t-1}$ x $market_t$				14.39
				(15.09)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
R^2	0.0603	0.0591	0.0603	0.0606
Observations	75897	75897	75897	75897

Table 7
Continued: Risk, Trading Frictions, and Limited Capital
Panel C: Limited Capital

Return $[t-1, t+1]$	Excl quarters ret$< -.05$	VIX		Earnings arb capital	
	(1)	(2)	(3)	(4)	(5)
$Surprise_{t-1}$	-1.060***	-0.968***	-1.004*	-0.916***	-0.855***
	(0.248)	(0.252)	(0.596)	(0.250)	(0.294)
VIX		0.0000564	0.0000550		
		(0.0000724)	(0.0000778)		
$Surprise_{t-1}$ x VIX			0.00161		
			(0.0266)		
Arb capital				-0.231	-0.207
				(0.282)	(0.293)
$Surprise_{t-1}$ x arb capital					-41.40
					(116.1)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes
R^2	0.0643	0.0646	0.0646	0.0602	0.0602
Observations	63730	70964	70964	67127	67127

Table 8
Strategic Timing of Earnings Announcements

This table tests whether the negative relation between return reactions and $surprise_{t-1}$ is driven by changes in the scheduling of announcements. $\Delta date$ is the difference between the day of the current earnings announcement and the previous year's same-quarter earnings announcement (e.g., for a firm announcing on March 15, 2004 that previously announced on March 12, 2003, $\Delta date = 3$). All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Return $[t-1, t+1]$	
	(1)	(2)
$Surprise_{t-1}$ x abs(Δ date) ≤ 5	-0.945***	
	(0.240)	
$Surprise_{t-1}$ x abs(Δ date) > 5	-0.716	
	(0.753)	
$Surprise_{t-1}$ x Δ date < -5		0.797
		(0.982)
$Surprise_{t-1}$ x abs(Δ date) ≤ 5		-0.945***
		(0.240)
$Surprise_{t-1}$ x Δ date > 5		-1.213
		(0.924)
Own $surprise_{it}$ controls	Yes	Yes
R^2	0.0608	0.0612
Observations	70091	70091

Table 9
Robustness to Different Measures of $Surprise_{t-1}$

This table shows robustness to alternative measures and sample restrictions. All variables and weights are as defined in Table 2, except for the following changes. Columns 1 and 2 present regressions that do not use any variables derived from analyst forecasts. We measure the salient surprise in $t-1$ as the value-weighted average of the return response to the $t-1$ earnings announcements of large firms above the 90th percentile of market capitalization, and do not control for the analyst-based measure of own earnings surprise (similar to the unconditional regressions in Columns 1-3 of Table 2). Column 1 uses the full sample for which we have return-based data and Column 2 limits the sample to observations for which we also have analyst-based surprise measures for both $surprise_{t-1}$ and the firm announcing today. In Column 3, $surprise_{t-1}$ is calculated as the volume-weighted mean earnings surprise of firms that announced on $t-1$ above the 90th percentile of volume (cutoff measured over the prior year) on the announcement day. In Column 4, $surprise_{t-1}$ is calculated as the weighted mean surprise of firms that announced on $t-1$ above the 90th percentile of market capitalization, with weights equal to the number of analysts covering the firm. Column 5 examines a measure of $surprise_{t-1}$ equal to actual earnings minus median forecast, without scaling by lagged price. Column 6 scales $surprise_{t-1}$ by the sum of the squared size weights of each firm comprising the weighted-mean calculation of $surprise_{t-1}$. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Return $[t-1, t+1]$					
	(1)	(2)	(3)	(4)	(5)	(6)
Return $surprise_{t-1}$, VW mean	-0.0520***	-0.0492**				
	(0.0197)	(0.0247)				
$Surprise_{t-1}$, volume-weighted			-0.285**			
			(0.130)			
$Surprise_{t-1}$, analyst-weighted				-0.915***		
				(0.218)		
$Surprise_{t-1}$, no price scaling					-0.0310***	
					(0.00667)	
$Surprise_{t-1}$, scaled std dev						-0.486***
						(0.130)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0172	0.0254	0.0580	0.0600	0.0596	0.0600
Observations	136056	74897	73472	75897	75923	75897

Table 10
Timing of Contrast Effects

This table provides further analysis of the timing of contrast effects. Panel A examine the impact of $t-3$, $t-2$, $t-1$, $t+1$, and $t+2$ salient surprises on return reactions to earnings announcements on day t. The dependent variable in Columns 1 and 2 is the return over the window $[t-3, t+1]$. The dependent variable in Columns 3 and 4 is the return over the window $[t-1, t+3]$. Column 2 limits the sample to own firm announcements released on Thursday and Friday while Column 4 limits the sample to Monday and Tuesday. Dummy variables are included for instances where there is a missing salient surprise of the indicated day. p-values are for the test of whether the $t-1$ coefficient is equal to the indicated coefficient. Panel B explores contrast effects within the same day. In Column 1, we re-estimate our baseline specification using $surprise_t$ instead of $surprise_{t-1}$. In Columns 2-6, we classify an earnings announcement as "AM" or "PM" based on whether it was released before market open or after market close. Column 2 and 3 regress the $[t, t+1]$ returns of firms that released PM announcements on the value-weighted surprises of large firms that released AM and PM announcements, respectively. Columns 4 and 5 repeat the analysis using firms with AM announcements. Column 6 measures the salient surprise as the surprise released by large firms in the previous half day (the same-day morning for PM announcers and the previous trading day afternoon for AM announcers). The firm's own earnings surprise is always excluded from the calculation of the salient surprise measure within a contemporaneous time window. In Panel C, the baseline specification is estimated separately for earnings announcements on each day of the week as indicated in the column labels. In Panel D, the baseline specification is estimated separately for each quarter of the year as indicated in the column labels. All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Further Lags and Leads

	Return $[t-3, t+1]$		Return $[t-1, t+3]$	
	(1)	(2)	(3)	(4)
$Surprise_{t-3}$	-0.223	-0.133		
	(0.227)	(0.266)		
$Surprise_{t-2}$	0.260	0.0664		
	(0.270)	(0.871)		
$Surprise_{t-1}$	-0.728***	-1.198**	-1.045***	-0.778**
	(0.247)	(0.480)	(0.279)	(0.314)
$Surprise_{t+1}$			0.0689	-0.143
			(0.393)	(0.494)
$Surprise_{t+2}$			-0.395	-0.263
			(0.379)	(0.681)
p-value: (t-3) = (t-1)	0.117	0.0398		
p-value: (t-2) = (t-1)	0.00894	0.216		
p-value: (t+1) = (t-1)			0.0219	0.278
p-value: (t+2) = (t-1)			0.172	0.487
Days	All	Th Fr	All	Mo Tu
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
R^2	0.0575	0.0741	0.0530	0.0577
Observations	75844	29376	75859	25408

Table 10
Continued: Timing of Contrast Effects

Panel B: Same-Day Contrast Effects

Own announcement time:	Today	PM		AM		AM or PM
	(1)	(2)	(3)	(4)	(5)	(6)
Others' announcement time:	Today	AM	PM	AM	PM	Prev halfday
Others' surprise	-0.587*	-1.263*	0.426	-0.417	-0.444	-0.479
	(0.307)	(0.664)	(0.694)	(0.408)	(0.343)	(0.375)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0558	0.0900	0.0805	0.0641	0.0586	0.0738
Observations	79886	19300	20059	21824	17899	36901

Panel C: Day of the Week

Own announcement:	Monday	Tuesday	Wednesday	Thursday	Friday	Tu-Th
	(1)	(2)	(3)	(4)	(5)	(6)
$Surprise_{t-1}$	-1.170**	-0.583*	-0.706	-1.005*	-2.578***	-0.686***
	(0.460)	(0.298)	(0.623)	(0.541)	(0.896)	(0.248)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0875	0.0702	0.0407	0.0735	0.104	0.0577
Observations	7743	17677	21083	23577	5817	62337

Panel D: Quarter of the Year

Quarter (calendar year)	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
$Surprise_{t-1}$	-1.331***	-0.857***	-0.757	-0.848*
	(0.483)	(0.330)	(0.739)	(0.456)
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes
R^2	0.0513	0.0714	0.0717	0.0594
Observations	16185	20935	18783	19994

Table 11
Heterogeneity

This table shows how contrast effects vary by the size, analyst coverage, and decade of the firm announcing today. In Column 1, $surprise_{t-1}$ is interacted with indicators for five quintiles for the size (as measured in $t-3$, using quintile cutoffs of the NYSE index in that month) of the firm announcing today. In Column 2, $surprise_{t-1}$ is interacted with indicators for the number of analysts covering the firm announcing earnings today (the number of distinct analysts that released forecasts in the past 15 days excluding day t and $t-1$). In Column 3, we estimate separate effects for each decade in the sample. All direct effects of size quintiles or number of analysts are included in the regression. All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Return $[t-1, t+1]$		
	(1)	(2)	(3)
$Surprise_{t-1}$ x size quintile 1	-0.527		
	(0.480)		
$Surprise_{t-1}$ x size quintile 2	-0.792*		
	(0.475)		
$Surprise_{t-1}$ x size quintile 3	-0.324		
	(0.443)		
$Surprise_{t-1}$ x size quintile 4	0.179		
	(0.304)		
$Surprise_{t-1}$ x size quintile 5	-1.028***		
	(0.255)		
$Surprise_{t-1}$ x (num analysts = 1)		0.0428	
		(0.607)	
$Surprise_{t-1}$ x (num analysts = 2)		-1.048**	
		(0.508)	
$Surprise_{t-1}$ x (num analysts $\geq$ 3)		-1.020***	
		(0.256)	
$Surprise_{t-1}$ x 1980s			-0.663
			(0.455)
$Surprise_{t-1}$ x 1990s			-1.024*
			(0.545)
$Surprise_{t-1}$ x 2000s			-0.542
			(0.346)
$Surprise_{t-1}$ x 2010s			-1.001**
			(0.462)
Own $surprise_{it}$ controls	Yes	Yes	Yes
R^2	0.0604	0.0604	0.0631
Observations	75897	75897	75897

Table 12
Industry Match

This table explores how contrast effects vary with the industry match between the firm announcing earnings today and the firm announcing in the previous trading day. $Surprise_{t-1}$ same ind is the salient earnings surprise in $t-1$, calculated using only firms in the same industry as the firm announcing today. $Surprise_{t-1}$ dif ind is the salient earnings surprise in $t-1$, calculated using only firms in a different industry as the firm announcing today. To make the magnitudes of the coefficients on the $t-1$ salient surprises comparable, we scale each salient surprise by the sum of the squared size weights of each firm comprising the weighted-mean calculation. Small (large) firm is a dummy variable equal to one if the $t-3$ size of the firm announcing earnings today is below (above) the median NYSE market capitalization in that month. Columns with "both $surprise_{t-1}$ non-missing" listed as Yes only include observations where same and different industry $surprise_{t-1}$ measures are non-missing. p-values are for the test of whether a given same-industry coefficient is equal to its different-industry analogue. All other variables and weights are as defined in Table 2. Standard errors are clustered by date and reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Return $[t+1, t-1]$	Full sample			Small firms		Large firms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Surprise_{t-1}$ same ind	-0.507***	-0.421***	-0.520***	-0.746***	-0.794***	-0.497***	-0.511***
	(0.180)	(0.133)	(0.184)	(0.228)	(0.239)	(0.184)	(0.189)
$Surprise_{t-1}$ dif ind	-0.489***	-0.171*	-0.321*	-0.440**	-0.214	-0.487***	-0.321*
	(0.161)	(0.104)	(0.184)	(0.206)	(0.248)	(0.166)	(0.189)
Both $surprise_{t-1}$ non-missing	No	No	Yes	No	Yes	No	Yes
Regression weights	Value	Equal	Value	Value	Value	Value	Value
p-value: same=dif	0.944	0.181	0.462	0.355	0.123	0.970	0.495
Own $surprise_{it}$ controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.0605	0.0682	0.0557	0.0833	0.0873	0.0595	0.0544
Observations	75897	75897	49343	33831	20847	42066	28496